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ART. XXIV.—*Origin of Coral Reefs and Islands;* by JAMES D. DANA.

[Continued from page 105.]

PART II. THE OBJECTIONS CONSIDERED.

THE objections to the Darwinian theory may be considered in the following order:

I. Darwin's insufficient knowledge of the facts bearing on the subject.

II. Subsidence not ordinarily a fact because methods of producing barrier reefs and atolls have been brought forward that do not require its aid.

III. The occurrence of cases of elevation in regions of atolls and barrier reefs inconsistent with the subsidence-theory.

IV. No ancient coral reefs in the geological series have the great thickness attributed by the subsidence-theory to modern reefs.

V. Other methods of explanation and their supporting evidence.

The adverse remarks directed against the idea of a sinking *continent* in the Pacific as the initial condition in the coral-reef subsidence are outside of the present discussion for the reason stated on the first page of this paper. In the following pages the objections are first explained, under the above-mentioned heads, and then follow, in paragraphs lettered *a*, *b*, *c*, etc., the writer's discussions of the several points.

I. *Darwin's insufficient knowledge of the facts.*

In the Address referred to in the opening page of this article, Dr. Geikie, speaking of Darwin, observes: "It should be borne in mind that, compared with more recent explorers, he did not enjoy large opportunities for investigating coral reefs." "He appears to have examined one atoll, the Keeling Reef, and one barrier-reef, that of Tahiti." "By a gradually widening circle of observations a series of facts has been established which were either not known, or only partially known, to Darwin."—The authors appealed to for the views that are presented as a substitute for Darwin's are Prof. Karl Semper, who has examined and described reefs of the Pelew and Philippine Islands; Dr. J. J. Rein, who has published on the physical geography of the Bermudas; Prof. Alexander Agassiz, who has written on the Florida reefs and others in that vicinity; and Mr. John Murray of the Challenger Expedition, whose investigations were made at Tahiti: all able men in science whether more learned or not than Darwin on the special subject under discussion. The facts from "a widening circle of observations" referred to comprise the physical and biological results of deep-sea exploration. The writer is mentioned as one of the "competent observers" who had given "independent testimony" in favor of Darwin's views after "at least equal opportunities of studying the subject," and as he has, in these later years, looked into the new facts, he has at least a claim to a hearing.

As to Darwin's knowledge, it appears to the writer that the apology offered in the above citations was not needed. In his detailed investigation of Keeling atoll—a good example of atolls and like all the rest in its principal features—and in his examination of the Tahitian reefs, followed up by a careful study of other atolls and reefs of the ocean through the maps and descriptions of former surveying expeditions, he had a broad basis for judgment and right conclusions. When the second edition of his work was published in 1874, many of the important facts from deep-sea exploration were already known; and later he learned of the more recent results; and he did not recant. A letter of his, of October 2nd, 1879, published by Mr. Semper, while admitting with characteristic fairness the interest of the facts collected by the latter, expresses his continued adherence to the opinion "that the atolls and barrier reefs in the middle of the Pacific and Indian oceans indicate subsidence."

The writer, as his expositor, may be excused for adding here that his own "independent testimony" was based on observations among coral reefs and islands in the Pacific during parts of three years, 1839, 1840 and 1841; that, besides working

among the reefs of Tahiti, the Samoan (or Navigator) Islands, and the Feejees (at this last group staying three months), he was also at the Hawaian Islands; and, in addition, he landed on and gathered facts from fifteen coral islands, seven of these in the Paumotu Archipelago, one, Tongatabu, in the Friendly Group, two, Taputeuea and Apia, in the Gilbert Group, and five others near the equator east of the Gilbert Group, Swain's, Fakaafo, Oatafu (Duke of York's), Hull and Enderbury Island.* The writer may, therefore, be acquitted of presumption if he states his opinion freely on the various questions that have been brought into the discussion by other investigators. Sympathising fully with the sentiment expressed in the words, "The example of Darwin's own candor and over-mastering love of truth remains to assure us that no one would have welcomed fresh discoveries more heartily than he, even should they lead to the setting aside of some of his work," and knowing that we are all for the truth and right theory, he has reason to believe that those who have been led to object to Darwin's conclusions will be pleased to have their objections reviewed by one who has a personal knowledge of many of the facts.

II. *Subsidence not ordinarily a fact because methods of origin have been brought forward that do not require its aid.*

It is urged that, while subsidence may have happened in several cases, it is not at all necessary to the making of barrier reefs and atolls; that "subsidence has been invoked because no other solution of the problem seemed admissible;" that the "solution" by subsidence "is only, an inference resting on no positive proofs."†

a. Darwin's usual methods were not such as these words imply, and we think that he was true to those methods in his treatment of coral island facts. Darwin can hardly be said to have "invoked" subsidence. Subsidence forced itself upon his attention. He saw evidence that it was a fact, and the theory came ready-made to him. The proof of subsidence from the relations in form, structure and history between atolls and the large barrier islands, like the Gambier Group, Raiatea, Bolabola and Hogoleu, scarcely admitted, he says, of a doubt; and other facts were all in harmony with it. This, his chief argument, with the enforcing evidence in my Report, (see §§ 4 and 10 of Part I of this paper) is not set aside and not mentioned in the Address from which the above sentences are cited.

b. Darwin observes that "from the nature of things it is scarcely

* These five islands are on the map of the Central Pacific accompanying Part I of this paper. Hull's Island, is "Sydney" of the writer's Expedition Report.

† Address, page 24.

possible to find direct proof of subsidence," recognizing the fact that subsidence, unlike elevation, puts *direct* testimony out of sight. But still it has left evidence which he perceived and thought convincing: and this stands, whatever virtue there may be in other explanations.

Moreover, we have now *direct* testimony for subsidence, from the facts brought forward (for another purpose) by Mr. Murray, as is set forth beyond.

III. *The occurrence of cases of elevation in regions of atolls and barrier reefs.*

The fact that elevated reefs and other evidences of elevation occur at the Pelews, a region of wide barrier reefs and atolls, has been presented by Prof. Karl Semper,* after a study of those islands, as an objection to the theory of subsidence; for we have thereby (in the words of the Address), "a cumbrous and entirely hypothetical series of upward and downward movements." Prof. Semper reports the existence of reefs raised 200 to 250 feet above the sea-level in the southern third of the larger of the islands, while the other two thirds exhibit evidence of but little, if any, elevation.

a. Such facts are of the same general character with those of other elevated reefs and atolls discussed in §§ 12, 13, 16 of Part I, and the same explanation covers them. The Pelew region is one of comparatively modern volcanic rocks and this renders local displacements a probability.

b. The occurrence of great numbers of large and small masses of coral rock, in some places crowded together, upon the *western* or *leeward* reef of the several Pelew islands, and of none on the eastern reef, is mentioned as evidence against subsidence and in favor of some elevation: because, Professor Semper says, the strongest wind-waves on the western side are too feeble to break off and lift on the reef so large masses, some of them (as his words imply rather than distinctly state) ten feet thick.

But the difficulty does not exist in fact; for earthquakes may have made the waves. The region just west of the Pelews is one of the grandest areas of active volcanoes on the globe. It embraces the Philippine Islands, Krakatoa and other volcanic islands of the Sooloo sea, Celebes, etc. The agents that could do the work were there in force. To the eastward, in contrast, lie the harmless islands of the Caroline Archipelago, mostly atolls, serving, perhaps, as a breakwater to the Pelews.

The small elevation referred to is therefore not proved by the

* First in 1868, Zeitschr. Wissensch. Zool., xiii, 558; additions in Die Philippinen und ihre Bewohner, Wurzburg, 1869; and still later in his "Animal Life" published in Appleton's International Scientific Series in 1881.

evidence adduced; and yet it may be a fact without affecting the theory of Darwin, as I have fully illustrated.*

It is important to have in mind that the coral-reef era probably covered the whole of the Quaternary and perhaps the Pliocene Tertiary also, and hence the local elevations that have taken place in the ocean were not crowded events of a short period.

Moreover, these local elevations in coral seas are spread over an area of 25,000,000 square miles. As an example of the long distances: the Paumotu Archipelago, consisting of more than eighty atolls and two barrier-islands, and covering about 450,000 square miles, contains only three or four atolls that are over twelve feet high; and of these, Metia is 250 feet in height, Elizabeth, 80 feet, Dean's probably where highest, 15 or 20 feet. Metia is one of the westernmost, near 148° 13′ W. and 15° 50′ S.: Dean's is 60 miles to the north-northeast of Metia, and Elizabeth is far to the southeast, in 128° W. and 25° 50′ S., or nearly 1450 miles distant from Metia. Locate these points on a continent, and Pacific distances and the length of Pacific chains of atolls will be appreciated.

IV.—*No ancient coral reefs have the thickness attributed by the subsidence-theory to modern reefs.*

An argument against the subsidence-theory is based by Prof. J. J. Rein † on the alleged fact that the thickness attributed to modern reefs is far beyond that of any such reefs in earlier time; that is, the thickness is unprecedented. The argument decides nothing. The question is one of geological fact, not to be settled by a precedent. Whether, then, there are precedents or not it is not necessary to consider.

Besides this, it implies a distinction between coral-made and shell-made rocks which does not exist. The coral-reef rock is largely made of shells, and the process of formation for a limestone of shallow-sea origin is essentially the same whether shells or corals are the predominant or the sole material. No thick formation of any kind of rock was ever made, or could be made, by shore or shallow-sea operations without a slowly continued subsidence or a corresponding change of water-level.

* Mr. Semper's objection to the theory of subsidence based on the coëxistence of all kinds of reefs in the Pelews, atoll, fringing and barrier, with no reefs about one island, and from the relative steepness of the submarine slopes on the east and west reefs of an island have been sufficiently met in Part I

† Dr. Rein's first memoir on Bermuda appeared in the Senckenberg Ber. naturforsch. Gesellschaft, 1869–70, p. 157, and the later in the Verhandlung des I. deutsch. Geographentages, 1881, Berlin, 1882. The above argument is from the latter paper, and is given here from the citation by Dr. Geikie, the publication not being accessible to the writer.

V.—*Other methods of explanation, and their supporting evidence.*

A. Mr. John Murray, one of the able naturalists of the Challenger Expedition, reports the following important results from soundings off northern Tahiti, made under his supervision and that of the surveying officer.*

Along a line outward from the edge of the barrier reef there were found: (1) for about 250 yards, a shallow region covered partly with growing corals, which deepened seaward to 40 fathoms; (2) for 100 yards, between the depths of 40 and 100 fathoms, a steeply but irregularly sloping surface which commenced with a precipice of 75° and had a mean angle exceeding 45°;† then (3) for 150 yards a sloping bottom 30° in angle; (4) then a continuation of this sloping surface, diminishing in a mile to 6°, at which distance out the depth found was 590 fathoms (3,540 feet). Over the area (2), or the 100 yards between 40 and 100 fathoms, the bottom was proved to be made of large coral masses, some of them "20 to 30 feet in length," along with finer debris; outside of this, of sand to where the slope was reduced to 6°; and then of mud, composed "of volcanic and coral sand, pteropods, pelagic and other foraminifers, coccoliths, etc."

These observations have great significance. They show (1) that the feeble currents off this part of Tahiti carry little of the coral debris in that direction beyond a mile outside of the growing reef; (2) that a region of large masses of coral rock and finer material occurs at depths between 240 and 600 feet; (3) that, a mile out, the bottom has the slope nearly of the adjoining land, and in this part is covered with the remains of pelagic life.

From the second of these facts—the great accumulation of coral blocks below a level of 240 feet—Mr. Murray draws the conclusion that, in the making of fringing, barrier and atoll reefs, the widening goes forward (*a*) by making first upon the submarine slopes outside of the growing reef a pile of coral debris up to the lower limit of living reef-corals; and then (*b*) by building outward upon this accumulation as a base.

He also announces, after speaking of other causes influencing the growth of corals, the more general conclusion that "it is not necessary to call in subsidence to explain any of the characteristic features of barrier reefs and atolls," and concludes that his views "do away with the great and general subsidences" appealed to by Darwin.

a. The widening-process, in the first conclusion, had previ-

* Proc. Edinburgh Roy. Soc., Session 1879-80, p. 505.

† Dr. Geikie gives in his paper a section of the soundings, "on a true scale, vertical and horizontal," and in it the upper steepest part of this 100 yards has a slope of about 75°.

ously been a part of the Darwinian theory; for, as stated in §10 (Part I), a fringing reef, where no subsidence is going on, widens above and steepens its seaward-slope, and it could do this only by the process described: that is, by building out upon a base of debris, or, more correctly, upon true coral-reef rock made by the gradual consolidation of the debris.*

b. The broader conclusion Mr. Murray does not sustain by a mention of special facts from the soundings, tending directly to meet the question of change of level, but by attempting to show that through the eroding action of currents and other means (as had been argued by Prof. Semper), in connection with the process already explained, reefs of all kinds can be made from submarine banks without aid from subsidence.

In this place I confine myself to the question as to the fact of subsidence. The only direct argument presented against subsidence is contained in the statement, that the very broad shore-plain of Tahiti shows that "the island has not in recent times undergone subsidence," and may indicate a slight elevation; and in this he sustains the earlier statement of my Report, which says (p. 293) that the broad shore-plain of Tahiti probably overlies in some parts the fringing reef, and (p. 300) the shore-plain, if built upon reefs, as I was assured, may afford proof of a rise of one or two feet." But this admission, as I have explained for other cases of local elevation, is in no way opposed to the theory of subsidence.

c. The kind of submarine slopes to be looked for off reefs is illustrated by the soundings, as Dr. Geikie indicates. But it is interesting to note that the facts, while very important, sustain instead of correcting those announced by earlier observers. Beechey and Darwin make the mean slope about 45°, and my Report says 40° to 50°. I have assumed for the slope of the bottom outside of the reef-limit the same angle as for the surface-slope of the island just above the water level: 5° to 8° off Tahiti, of which 5° is accepted as most correct, and 3° to 5° off Upolu;† and the assumption as regards Tahiti is sustained by the Challenger soundings. My Report states (from the Expedition surveys) that off Upolu, the bottom "loses more and more in the proportion of coral sand till we finally reach a bottom of earth," and introduces this as an argument against the indefinite drifting of coral sands into the deep ocean;‡ and this argument the Tahiti soundings sustain.

With reference to the occurrence off some shores of precipitous submarine slopes, the Challenger soundings give definite facts as to one case. It leaves undisturbed the previously re-

* My Expedition Geological Report, pp. 131, 132, where figures are given illustrating the effect of widening.

† Ibid., page 47. ‡ Ibid., page 154.

ported cases of like steepness at greater depths; for example, the sounding of Captain Fitzroy at Keeling atoll (while Darwin was there), 2200 yards from the breakers, in which no bottom was found at a depth of 1200 fathoms, but the line was partly cut at a depth between 500 to 600 fathoms; the sounding by the Wilkes Expedition off Clermont Tonnerre (Paumotu Archipelago), where the lead brought up an instant at 350 fathoms, and then dropped off again, descended to 600 fathoms without reaching bottom, and came up bruised, with small pieces of white and red coral attached; a sounding by the same Expedition, a "cable's length" from Ahii, in which the lead struck a ledge of rock at 150 fathoms and brought up finally at 300 fathoms.* All the older soundings need to be repeated; but there must be enough truth in those quoted to warrant the remark that the force of Darwin's argument for subsidence from the steepness of the submarine slopes about atolls is not weakened by the Challenger results.

d. But the chief interest of the Challenger soundings consists in their affording "direct" proof, "positive" proof, of *much subsidence;* a kind of proof that subsidence sinks out of sight, and which soundings may yet make available in many similar cases.

That belt of coarse debris—including "masses 20 to 30 feet" long—was found over the steeply sloping bottom at depths between 240 and 600 feet. These depths are far below the limit of forcible wave-action. They are depths where the waters, however disturbed above by storms, have no rending and lifting power, even when the bottom is gradually shelving; depths, in this special case, against a slope which for 100 yards is 75° in its upper part, and in no part under 45°, the vertical fall being 360 feet in the 100 yards. Strokes against the reef-rock thus submerged, and under such conditions, would be extremely feeble. Waves advancing up a coast, whether storm-driven waves or earthquake-waves, do little rock-rending below the depth to which they can bare the bottom for a broadside plunge against the obstacle before them, although the velocity gives them transporting power to a greater depth. It is the throw of an immense mass of water against the front, with the velocity increased by the tidal flow over a shelving bottom,—the rate sometimes amounting, according to Stevenson, to 36 miles an hour or 52·8 feet a second,—together with the buoyant action of the water, that produces the great effects.

A vertical surface below the sea-level of 20 feet made bare for the broadside stroke is probably very rarely exceeded even

* Ibid., page 55.

in the case of earthquake-waves; and with storm-waves, or recorded earthquake-waves, the displacement of the water at a depth of 240 feet would be at the most only a few inches. I saw on atoll reefs no upthrown masses of coral rock over ten feet in thickness and twenty feet in length or breadth. It is therefore plainly impossible that such a belt of debris should have been made at its present level, or even at a depth of 20 feet; and hence the debris affords *positive proof of a large subsidence during some part of the reef-making era.*

The existence of the belt of debris may be explained as follows: If the reef now at a depth of 240 feet were at the sea-level as the sea-level reef, and subsidence were not in progress for a period, the very steep front of the reef now just below the 240-foot level might have resulted from the widening that would have gone forward. And, under such conditions, the action of the occasional extraordinary waves might have torn off masses from the front which would have tumbled down the steeply sloping surface until the belt of debris had been formed. Then, with a renewal of the slow subsidence, the thickening of the reef would have been resumed and gone on to its final limit, and the rendings of the great waves found lodgment at higher levels. The masses now on atoll reefs must be from comparatively recent upthrows.

This *direct* evidence of subsidence from Tahiti renders it reasonable to make subsidence in atoll-making a general truth. It is nevertheless desirable that facts of the kind should be multiplied. The abrupt descent in the submarine slopes of reefs detected by Fitzroy at a depth below 3000 feet, and those reported by the Wilkes Expedition at depths of 2100 and 900 feet, seem to indicate a similar rest at the sea level and consequent reef-widening, in the course of a progressing subsidence; and proof of this may yet be found in belts of coarse coral-rock debris at the foot of the precipices. Such a period of rest would lead to the forming of submarine precipices in different regions contemporaneously at different depths according to the rate of subsidence of the part of the subsiding area.

B. From facts observed about the Florida reefs, Lieutenant E. B. Hunt, U. S. N., announced, in 1863,* the conclusion that these reefs had received their westward elongation through the westward "sweep" of an eddy current to the Gulf Stream. The subject, nearly twenty years afterwards, was more thoroughly investigated by Mr. Alexander Agassiz, and the same

* This Journal, II, xxxv, 197.

conclusion reached.* Mr. Agassiz made also another important observation—that this current is an abundant carrier of marine life for the feeding of the coral animals, and so accelerates the coral growth and accumulation in its direction. Combining with these effects others considered beyond, Mr. Agassiz expresses, like Mr. Murray and Mr. Semper, the further conclusion, that all kinds of reefs, atoll, fringing and barrier, may be made without aid from subsidence.

a. The facts, presented by Lieutenant Hunt, and more fully by Mr. Agassiz with regard to the effects of the eddy current of the Gulf Stream, show that coral reefs may be elongated, and also that inner channels may be made, by the drifting of coral sands. But the action with coral sands is essentially the same as with other sands; and illustrations of this drifting process occur along the whole eastern coast of North America from Florida to Long Island. We there learn that drift-made beaches run in long lines between broad channels or sounds and the ocean; that they have nearly the uniform direction of the drift of the waters, with some irregularities introduced by the forms of the coast and the outflow of the inner waters which are tidal and fluvial and have much strength during ebb tide. The easy consolidation of coral sands puts in a peculiar feature, but not one that affects the direction of drift accumulation.

b. The great barrier reef off eastern Australia, a thousand miles long, has some correspondence in position to the sand-reefs off eastern North America. But it is full of irregularities of direction and of interruptions, and follows in no part an even line. In the southern half, it extends out 150 miles from the coast and includes a large atoll-formed reef; in the northern half, the barrier while varying much in course is hardly over 30 miles from the land. There is very little in its form to suggest similarity of origin to the drift-made barriers of sand.

c. In the Pacific Ocean, the trends of many of the coral island groups, and of the single islands, do not correspond with the direction of the oceanic currents, or with any eddy currents except such as are local and are determined by themselves.

*On the Tortugas and Florida Reefs, by A. Agassiz, Trans. Amer. Acad., xi, 1883.

Professor Louis Agassiz's account of the Florida reefs was published in the U. S. Coast Survey Reports of 1851 and 1866, and reproduced in vol. vii of the Memoirs of the Museum of Comparative Zoology. It gives an excellent description of the Florida reefs, and of the action of boring animals and other injurious agents on corals, and reaches the conclusion that the reef has been raised to its present level and thickness by wave and current action, without the aid of elevation or subsidence. The argument is based on such observations as could be made over the surface of the reefs and the adjoining sea-bottom, and bears on the question of the necessity of subsidence and not on the fact of subsidence.

Near longitude 180°, as the map of the Central Pacific (see Part I) illustrates, the equator is crossed by the long Gilbert (or Kingsmill) Group, at an angle with the meridian of 25° to 30°, and not in the direction of the Pacific current which is approximately equatorial. This obliquely crossing chain of atolls is continued northward in the Ratack and Ralick Groups (or the Marshall Islands), making in all a chain over 1200 miles long; and, adding the concordant Ellice Islands on the south, and extending the Ratack line to Gaspar Rico its northern outlier, the chain is nearly 2000 miles long. Nothing in the direction of the long range, excepting local shapings of some of the points about the atolls, can be attributed to the Pacific currents. Moreover, the diversified forms of the atolls have no sufficient explanation in the drift process.

d. Further: drifting by currents may make beaches and inner channels whether subsidence is going on in the region or not, and are not evidence for, or against, either a movement downward or upward. Sandy Hook, the long sandy point off the southern cape of New York harbor, has been undergoing (as the U. S. Coast Survey has shown) an increase in length, or rather variations in length, through the drifting of sands by an outside and an inside current; and this is no evidence that Professor G. H. Cook is wrong in his conclusion that the New Jersey coast is slowly subsiding.

e. But even *in this region of Florida we have strong evidence of a great subsidence during the coral-reef era*, and all the subsidence that the Darwinian theory demands.

In a very valuable paper by Mr. Agassiz, published in 1879 in the Bulletin of the Museum of Comparative Zoology,* the author points out that the South American continent, in comparatively recent geological times, had connection with the West India islands through two lines: (1) one along a belt from the Mosquito Coast to Jamaica, Porto Rico and Cuba; and (2) the other through Trinidad to Anguilla, of the Windward Islands. He sustains the conclusion by a review of the soundings made by the Steamer Blake under the command of J. R. Bartlett, U. S. N., and a consideration of the facts connected with the distribution of marine and terrestrial species. As the soundings show, the former of the two connections requires for completeness an elevation of the region amounting to 4060 feet over the part south of Jamaica, 4830 feet between Jamaica and Hayti, and 5240 feet between Hayti and Cuba. The other line of connection requires an elevation of 3450 feet. An open channel, as he observes, would thus be left between Anguilla and the Virgin Islands, where there is now a depth of 6400 feet. The close relations in the existing fauna

* An abstract of the paper is contained in this Journal, III, xviii, 230, 1880.

of the Gulf to that of the Pacific waters prove that it continued to be a salt-water gulf through the era of elevation.

Mr. Agassiz infers that the connection of the West India Islands with South America existed before the Quaternary era. But there are other facts which seem to prove that it was continued into, or at least was a fact, in the Quaternary.

The opinion as to a connection of the Windward Islands with South America in the Quaternary was presented by Prof. E. D. Cope in 1868, and earlier, as he states, by Pomel, on the ground of the discovery in the caves of Anguilla of a species of gigantic Rodent related to the Chinchilla, as large as the Virginia deer, and nearly equaling the Quaternary *Castoroides* of Ohio.* Further, De Castro, as cited by Dr. J. Leidy in his "Mammalian Fauna of Dakota and Nebraska," 1869, announced, in 1865, a gigantic sloth of the "Quaternary," from Cuba, which he referred to the genus *Megalonyx*, and Dr. Leidy named *Megalocnus rodens*, proving a Quaternary connection between the continent and Cuba.

The fact of an elevated condition of the region sufficient to make Cuba and Anguilla part of the continent during the earlier Quaternary, if not in the Pliocene also, is thus made quite certain. This is fully recognized by Wallace.† Such a condition could hardly have existed without a large elevation also of Florida, though probably not, as Mr. Agassiz holds, to the full amount of the depression between it and Cuba—nearly 3000 feet—because Cuba is most closely related in fauna to South America. The subsidence which brought the region to the present level was consequently within the coral-reef period. It is hence hardly to be doubted that the making of the Florida, Bahama and other West India coral reefs was going on during the progress of a great subsidence. None of the facts mentioned by observers are opposed to this view.

It is of interest to note here that on Cuba and Jamaica there are *elevated* coral reefs, the highest on Cuba 1000 feet above the sea, according to Mr. Agassiz, and probably at one point 2000, according to Mr. W. O. Crosby's observations,‡ and on Jamaica 2000 feet, according to Mr. Sawkins; indicating that there have been upward movements subsequent to the down-

* Proc. Philad. Acad. Nat. Sci., 1868, 313, and Proc. Philad. Amer. Phil. Soc., 1869, 183; also Smithsonian Contributions to Knowledge, 30 pp. 4to with 5 plates, Washington, 1883. The last paper (prepared in 1878) contains descriptions of the following species from the Anguilla bone-cave. *Amblyrhiza inundata* Cope (the large Rodent announced in 1869), *A. quadrans* Cope, *A. latidens* Cope, an Artiodactyl apparently of the *Bovidæ* and a little smaller than *Ovis aries*. With them was obtained an implement ("a spoon-shaped scraper or chisel") made of the lip of the large *Strombus gigas*.

† Geograph. Distrib. of Animals, ii, 60, 78.

‡ Proc. Boston Soc. Nat. Hist, xxii, 124, 1882, and in abstract in this Jour. xxvi, 148, 1883.

ward. Mr. Crosby argues that the great thickness of the now elevated reefs could have been produced only "during a progressing subsidence," so that "we have apparently no recourse but to accept Darwin's theory."

C. It has been urged by Mr. Semper, Dr. J. J. Rein, Mr. A. Agassiz, Mr. Murray, Dr. Geikie and others, that since the growing calcareous deposits of the sea-bottom are slowly rising toward the surface by successive accumulations of the shells and other debris of marine species, they may have been built up locally in various regions of the deep seas (as they actually are now about some islands) until they were near enough to the surface to become next a plantation of corals; and that in this way, atolls became common within the area of the tropical oceans. The method is regarded as setting aside subsidence.

a. The advocates of this hypothesis have not pointed to such a mound now approaching the ocean's surface on the western border of the Gulf Stream, where the depth over the remarkably luxuriant region is least; and none over any part of the tropical Pacific. It is suggested that the Chagos Bank may be one example; but it is not known to be so. Professor Semper states that he found evidence of pelagic life instead of modern corals in the lower part of the elevated reefs of the Pelews. Dr. Geikie cites from letters by Dr. Guppy in Nature of Nov. 29, Dec. 6, 1883, and Jan. 12, 1884, the fact that in elevated reefs on the Salomon Islands, 100 to 1200 feet high, the coral rock forms a comparatively thin layer over impure earthy limestone abounding in foraminifers and other pelagic organisms, such as Pteropods. Such observations have great interest, but they only prove that, in coral-reef seas, corals will grow over any basis of rock that may offer where the water is right in depth, and do not nullify any of the evidences of subsidence. This point should be kept before the mind in all future study of coral-reef regions. Borings in coral islands, as recommended on a former page, are the true means of investigating it.

b. The old hypothesis that atolls may have been built upon the summits of submerged mountain-peaks or volcanic cones at the right distance under water for growing reef-corals, or, if not at the right level, brought up to it by other organic depositions, or down to it by abrasion, is urged by Mr. Murray.

This writer observes that "the soundings of the Tuscarora and Challenger have made known numerous sub-marine elevations: mountains rising from the general level of the ocean's bed at a depth of 2500 or 3000 fathoms up to within *a few hundred fathoms* of the surface." But "a few hundred fathoms," if we

make few equal 2, means 1200 feet or more, which leaves a long interval yet unfilled.*

It is also urged that some of the "emerged volcanic mountains situated in the ocean basins" may have been wholly swept away and left with a few fathoms of water above them. But this is claiming more from the agents of erosion than they could possibly have accomplished, as the existence of an atoll in the ocean and the examples on coasts of wave and tidal action prove.

D. To give completeness to the hypothesis which makes barrier and atoll islands out of sub-marine banks (whether these banks have a basis of volcanic or other rocks, or of calcareous accumulations), it is necessary to show that the waters of the waves and currents can make barrier islands and atolls out of such banks without subsidence; and explanations to this effect have been given.

It is urged, in agreement with Darwin, that the outer portions of reefs increase faster than the inner, owing to the purer water about them and the more abundant life for food; that the inner parts are not only at a disadvantage in these respects but suffer also from coral debris thrown over them. They add to these causes of unequal growth mentioned by Darwin, the solvent and abrading action of the waters.

It is hence concluded that, under these conditions, the simple bank of growing corals may have a depression made at center, which, as the process continues, will become a lagoon basin, and the reef, thereby, an atoll with a shallow lagoon; that the atoll, so begun, may continue to enlarge through the external widening of the reef and the further action of current-abrasion and solution within; or, in the case of fringing reefs, that the change may go on until the reef has become a barrier-reef with an inner channel and inner reefs. It is admitted that subsidence may possibly have helped in the case of the deepest lagoons.

Dr. Geikie expresses his opinion on the subject thus: "As the atoll increases in size the lagoon becomes proportionally larger, partly from its waters being less supplied with pelagic

* The actual depths over the elevations in the Tuscarora section between the Hawaian Islands and Japan, numbering them from east to west, are as follows: 1, 11,500 feet; 2, 7500 feet; 3, 8400 feet; 4, 12,000 feet; 5, 9000 feet (this seven miles west of Marcus Island); 6, 9600 feet. Whether ridges or peaks the facts do not decide; probably the former. No. 1 has a base of 185 miles with the mean eastward slope 40 feet per mile (=1:132) and the westward 128 feet per mile. No. 2 has a breadth of 396 miles, with the mean eastern slope mostly 37 feet per mile, but 51 feet toward the top, and the westward, 55 feet per mile (=1:96). No. 3 was the narrowest and steepest, it being about 100 miles broad at base and having the mean eastern slope 192 feet per mile and the mean western 200 feet.

food, and therefore less favorable to the growth of the more massive kinds of corals, partly from the injurious effects of calcareous sediment upon coral growth there, and partly also from the solvent action of the carbonic acid of the sea-water upon the dead coral."

Mr. Semper gives examples of the effects of currents at the Pelew Islands, stating that by striking against or flowing by the living corals they make the reef grow with steeper sides and determine its direction, and urging that abrasion and solution have made not only the deep lagoon-like channels, but the deeper channels between the islands. He holds that in Kriangle, which he describes as a true atoll with no channel leading into the lagoon from the sea, that the lagoon may have been "the result of the action of currents on the porous soil during a period of slow upheaval.* He says, further, that the large channel in the main island of the group "forty fathoms deep and many miles wide," "finds an easy explanation on the assumption of an upheaval;" it became "wider in proportion as the enclosed island consisting of soft stone [tufa] was gradually eaten away, and during slow upheaval it would continue to grow deeper in proportion as the old porous portions of the reef and the rock in which it was forming were more and more worn down by the combined action of boring animals and plants, and of the currents produced by the tides and by rain." Mr. Semper refers to the dead depressed tops of some masses of Porites near tide-level as the effects of the deposit of sediment over the top of the living coral and of erosion by the waves and exposure to rains while the sides continued to grow; and the fact is made an example on a very small scale of atoll-making. Other examples of the action of currents, sediment, boring species, and the solvent action of carbonic acid in the waters, are mentioned by Mr. Agassiz, in his excellent account of the "Tortugas and Florida reefs."

a. The theory, if satisfactory, accounts not only for the origin of an atoll, but for the origin of atolls of all sizes, shapes and conditions, and for great numbers of them in archipelagos and chains; not only for channels through fringing reefs, like those that abrasion in other cases makes, but for all the irregular outlines of barriers, for the great barriers reaching far away from any land, and for the positions and indented coasts of the small included lands. Is it a sufficient explanation of the facts?

b. The currents that influence the structure of reefs are: (1), the general movement or drift of the ocean, in some parts varying with seasonal variations in the winds; (2), the currents

* Animal Life," pp. 269, 270.

connected with wave-action and the inflowing tide over a shelving bottom; and (3), the currents during the ebb, flowing out of channels; together with (4) counter-currents. Each region must have its special study in order to mark out all the local effects that currents occasion. Such effects are produced whether a secular subsidence is in progress or not, and hence a particular review of the subject in this place is unnecessary.

The shaping of the outside of the reef and the determination of the width and level surface of the shore-platform are due chiefly to the tidal flow and the accompanying action of wind-waves, as explained in §17 of Part I.*

The current that accompanies the ebb is locally the strongest. Owing to the great width of many barrier reefs and of the channels and harbors within them, the tide flows in over a wide region. At the turn in the tide the waters escape at first freely

* Since the first part of my paper was published I have observed in an article by Mr. A. R. Hunt, in the Scientific Proceedings of the Royal Dublin Society, iv, 254, January, 1885, the remark, referring to a statement of the above fact in my Manual of Geology, that the "statement though strictly in accordance with Mr. Russell's theory, has so far as I can ascertain, no foundation in fact." The statement, as I have said (and as I illustrate in my Geology) was but the statement of a fact observed by me first in 1839 on the coasts of Australia and New Zealand, without a thought of any theory; and part of the explanation is overlooked by Mr. Hunt. I *observed* that the first waters of the incoming tide swelled over the sandstone platform (which was a hundred yards or more wide off the Port Jackson heads), and became thus a protector of the sandstone *platform* from breaker strokes; and that the lower part of the sandstone *bluff* to a height a little above high tide, was hollowed out by the strokes of the breakers. A similar erosion near high tide level of the great coral masses standing on the coral-rock platform of atolls I also observed while among the Paumotu Islands. Prof. A. E. Verrill informs me that he has seen examples of the same action on a grand scale about the island of Anticosti in the Gulf of St. Lawrence. The observations do not appear to me to be at variance with the principles laid down in Mr. Hunt's valuable paper; they require only his recognition of a tidal effect which he does not fully consider, and which British seas cannot illustrate.

To produce a platform, (1) the rock-material exposed to the flow of the tide and the breakers must be firm enough to resist wear during the early part of the flow, and at the same time soft enough to allow the striking breakers to cut into the base of the bluff, or shear off the projecting ledge; and (2) the region must not be one of very high tides or stormy seas, for, in such regions of forceful waves and tides, the movements are too often of the destructive kind through the whole continuance of the flow, leaving no chance for the protection a platform needs. Loose sand-deposits are too soft; they are worn off below the sea-level and changed in surface by storms; but some firmer kinds may make a low-tide flat in a bay where the tides are small. Coral-reef rock, the material of the atoll platform, has the hardness and solubility, in carbonated sea-water, of ordinary limestone. The rock of the Port Jackson Heads is a friable sandstone. At the Bay of Islands, New Zealand, the platforms occur in an argillaceous rock, which becomes soft and earthy above by weathering, but is unaltered and firm below because kept wet (loc. cit. p. 442). At the Paumotus the tides are two to three feet high, and the platform usually 100 yards or more wide; at the Phœnix Group the tides are five to six feet high and the platform mostly fifty to seventy yards wide; at the Port Jackson Heads, the ordinary tides are six feet high and the platform fifty to one hundred and fifty yards wide; at the Bay of Islands (in the sheltered waters of the bay), the tides are eight feet high and the platform is under thirty yards wide.

over the same wide region; but, with a tide of but two or three feet, there is but little fall before the reef—which lies at low tide level and a little above it—retards it by friction; and thus escape by the open entrances is increased in amount and in rate of flow. The facts are the same in atolls where the lagoons have entrances.*

c. Examples of massive corals having the top flat, or depressed and lifeless, while the sides are living, are common in coral-reef regions, wherever such corals are exposed to the deposition of sediment, and where they have grown up to the surface so that the top is bare above low tide. A disk of Porites, having the top flat and the sides raised (owing to growth) so as to give it an elevated border, is figured on plate LV. of my Report on Zoophytes. Many such were found in the impure waters of a shore reef at the Feejees. At Tongatabu one flat-topped mass of Porites was twenty-five feet in diameter; and both there and in the Feejees, others of Astræids and Meandrinas measured twelve to fifteen feet in diameter.

Over the dead surfaces, as Mr. Semper observes, the coral may be eroded by the solvent action of the waters, and especially where depressions occur to receive any deposits, and boring animals may riddle the coral with holes or tubes. But generally the erosion is superficial; the large masses referred

* The currents of the tropical Pacific Ocean are of very unequal rate in its different parts, and very feeble in the Paumotu Archipelago and the Tahitian and Samoan regions. Capt. Wilkes reports that in the cruise of the Expedition through the Paumotu Archipelago to Tahiti, a distance of a thousand miles, during a month from August 13 to September 13, 1839, the drift of the vessels was only 17 miles; and that during fourteen days in the first half of October, between Tahiti and Upolu of the Samoan group, nearly 1800 miles, the drift was only 43 miles.

The Challenger, on her route from the Hawaian Islands to Tahiti, found, between the parallel of 10° S. and Tahiti, "the general tendency of the current westerly, but its velocity variable;" between the parallel of 10° S. and 6° N., the direction was westerly with "the average velocity 35 miles per day, the range 17 to 70 miles per day," the maximum occurring along the parallel of 2° N. Farther west, about the Phœnix group, the equatorial current, as described by Mr. Hague (loc. cit., p. 237), has "a general direction of west-southwest and a velocity sometimes exceeding two miles per hour." At times it changes suddenly and flows as rapidly to the eastward. The drifting of the sands about Baker's Island (in latitude 0° 13′ N., longitude 176° 22′ E.) has much interest in connection with this subject of current action, and the facts are here cited from Mr. Hague's paper. The west side of the little island ($1 \times \frac{3}{4}$ m. in area) trends northeast, and the southern east-by-north, and at the junction a spit of sand extends out. During the summer the ocean swell, like the wind, comes from the southeast, and strikes the south side; and consequently the beach sands of that side are drifted around the point and heaped up on the western or leeward side, forming a plateau along the beach two or three hundred feet wide, and eight or ten feet deep over the shore platform. With October and November comes the winter swell from the northeast, which sweeps along the western shore; and in two or three months the sands of the plateau are all drifted back to the south side, which is then the protected side, extending the beach of that side two or three hundred feet. This lasts until February or March when the operation is repeated.

to showed little of it. Such dead surfaces in corals are generally protected by a covering of nullipores and other incrusting forms of life, and the crusts usually spread over the surfaces *pari passu* with the dying of the polyps.

d. Every stream, says Mr. Semper (when explaining, as cited on a preceding page, the origin of the deep channel of the large Pelew island, whose depth is "35 to 45 fathoms"), "has a natural tendency to deepen its bed." But there is a limit to this action. The eroding or deepening power of a stream through abrasion and transportation is null or nearly so below the level of its outlet. A basin or channel 45 fathoms (270 feet) deep with an outlet of much less depth could not be deepened by such means or protect itself from shallowing. The depth of the outlets is not stated except that they are said to be ship-channels. Moreover, with a tufa bottom, solution could not contribute to the removal, since carbonated waters, although decomposing the tufa, dissolve very little of its ingredients. An elevation in progress would result in making of the channel a closed lake and finally dry land.

For the same reason, the small atoll, Kriangle, having, as described, a *closed* lagoon, could have no deepening of the lagoon from abrasion by tidal currents or wave-action during the progress of an elevation. And if a lagoon have an outlet, the rapid current of the ebb would be confined to the narrow passage-way and a portion of the bottom near it; through the larger part of the lagoon, as in any other lake, the waters would have scarcely perceptible motion, and therefore slight energy for any kind of work. Hence a lagoon would lose very little by this means, and shallowing would go on unless there were great loss through the *solvent action* of the waters. An elevation would only hurry the shallowing and end in emptying the lagoon.

e. Erosion through solvent action is promoted by the presence in the waters both of carbonic acid and organic acids. The material within reach of the tides or waves exposed to this action is dead corals and shells, or their debris, and bare coral rocks, occurring over: (1) the outer region of living corals and for a mile or so outside; (2) the shore platform and the reef, bare at low tide, on which there is comparatively little living coral; and (3) the lagoon basin. There is nothing in the material within the lagoon to favor solution more than in either of the other two regions; in fact, the platform and bare reef are most exposed to the action because of the small amount of living corals over them. The outside waters take up what they can through the carbonic acid they contain, and supply thereby the wants of the lime-secreting polyps, shells, etc., and carry on the process of solidification in the debris; the same waters move on over

the atoll reef and take up more lime as far as the acid ingredient is present; and then they pass to the lagoon for work similar to that outside, with probably a diminished amount of free carbonic acid, on account of the loss over the reef-ground previously traversed.

The lagoon-basin is not, therefore, the part of the atoll that loses most by solution, any more than by abrasion and transportation. The outer reefs suffer the most; and yet, if the island is not subsiding at too rapid a rate, they keep extending and encroaching on the ocean, instead of wasting through the drifting into the ocean at large of calcium carbonate in grains and solution; and the shore-platform also preserves its unvaried level notwithstanding the daily sweep of the tidal floods, and the holes that riddle its outer portion.

The remark: "It is a common observation in atolls that the islets on the reefs are situated close to the lagoon shore;" such "facts point out the removal of matter which is going on in the lagoons and lagoon channels,"* I know nothing to sustain. The width of the shore-platform on the seaward side is always greater than that on the lagoon side; but the outside shore-platform has its width determined by tidal and wave action, and this action is powerful on the ocean side, and feeble on the lagoon side; it produces a high coarse beach on the outside as the inner limit of the platform, and a finer, lower and much more gently sloping beach on the inside. The amount of erosion is far greater, as it should be, on the side of the powerful agencies.

f. The loss to the lagoon by abrasion and solution *is reduced to a minimum, in the majority of atolls, by the absence of lagoon entrances*, which leaves them with only concealed leakage passages for slow discharge.

Nine-tenths of atolls under six miles in length (or in longer diameter), half of those between six and twenty miles, and the majority of all atolls in the Pacific ocean, have no entrances to the lagoon a fathom deep; and the larger part of those included in each of these groups have no open entrances at all.

For evidence on this subject, I refer to the Wilkes Expedition Hydrographic Atlas. This atlas contains maps of nearly sixty coral islands from the surveys of its officers, drawn on a large scale (one or two miles, rarely four, to the inch).

Out of the number, nine, ranging from 1½ to 3 English miles in the longer diameter of the reef, *have no lagoon*, but only a small depression in its place; two of these take in water at high tide, and the rest are dry.

Of those *under six miles in length having lagoons*, seventeen in number, sixteen are represented as having *no entrances to the*

* Mr. Murray, loc. cit., p. 515.

lagoon at low tide; and the one having an entrance is 5×4 miles in size. The smallest is about a mile in diameter.

Of those that are *six miles or over* in length, twenty-nine in number, seventeen have channels and twelve have none. Those having channels are mostly over ten miles in length. A list of them is here given with their sizes, and also the proportion of the reef around the lagoon which is under water above third tide, and bare at low tide, a feature of much interest in this connection.

Ellice Group.—Depeyster's: 6×6 m.; three-fourths of the encircling reef bare. Ellice's: 9×5 m.; three-fourths bare.

Gilbert Group.—Apia: 17×7 m.; half bare. Tarawa: 21×9 m.; half bare. Taritari: 18×11 m.; two-thirds bare. Apamama: 12×5 m.; half bare. Taputeuea; west side mostly submerged.

Marshall Islands (northern).—Pescadores: 10×8 m.; four-fifths bare. Korsakoff: 26m.; four-fifths bare.

Paumotus.—Peacock: 15×7 m.; nearly all wooded. Manhii: 13×5 m.; nearly all wooded. Raraka: 6×9 m.; three-fourths wooded. Vincennes: 13×9 m.; mostly wooded. Aratica: 18×11 m.; three-fifths bare. Tiokea: 18×4 m.; two-thirds wooded. Kruesenstern's: 16×10 m.; mostly wooded. Dean's (or Nairsa): 53×18 m.; half or more bare.

g. The absence of open channels in so large a proportion of lagoons, and especially in lagoons of the smaller atolls, appears to be fatal to the abrasion-solution theory. The method of enlarging atolls through currents and solution can act only feebly if at all where waters have no free outlet; and this is eminently so with the smaller atolls which have been assumed by the theory to be most favorable in purity of water and in abundant life for progress; if the small cannot grow, the large lagoons cannot be made from them by the proposed method.

Reverse the method, letting the large precede the small (as under the subsidence theory), and then we have a consistent order of events. We have large atoll reefs with several large entrances (like the great barrier reef about a high island in this and other respects) gradually contracting, and the entrances concurrently narrowing through the growing corals and the consolidating debris, in spite of the efforts of abrasion and solution to keep them open and make them deeper; and, afterwards, the atoll becoming still smaller until the entrances close up; and finally the lagoon-basin is reduced to a dry depression with nothing of the old sea-water remaining except, perhaps, some of its gypsum.

h. Instead of small lagoons having the purest waters, the reverse is most decidedly and manifestly the fact, and this accords with the reversal in the history just suggested. Since

atolls of middle and larger size commonly have one third to two-thirds of the encircling reef covered with the sea at one-third tide, making the ocean and lagoon for more than half the time continuous, the large lagoon in such a case has as pure water as the ocean, and commonly as good a supply of food-life, and sometimes as brilliant a display of living corals. But in the smaller atolls, the area of the lagoon has little extent compared with the length and area of the encircling reef; coral sands and other calcareous material consequently have possession of the larger part of the bottom, and the waters, since they are less pure than those outside, contain fewer and hardier kinds of corals and less life of other kinds. They are exposed, also, to wider variations of temperature than the outer, with injury to many species, and at lowest tides may become destructively overheated by the midday sun, as many a plantation of corals with dead tops for a foot or more bears evidence. In the smallest atolls, the lagoons are liable also to alternations of excessive saltness from evaporation and excessive freshness from rains, and consequently no corals can grow inside, though still flourishing well in the shallow sea about the outer reef. The above are the facts, not the suggestions of theory.

i. We read: "So great is the destructive and transporting influence of the sea under the combined or antagonistic working of tides, currents and wind-waves that the whole mass of the reef, as well as the flats and shoals inside, may be said to be in more or less active movement."* This description of the Tortugas reefs is not applicable to the atolls of the Pacific. Notwithstanding the testimony of Captain Beechey and others about occasional catastrophes—which are mostly catastrophes to the islets and banks within the lagoons—I was led to look upon a coral island as one of the most stable of structures. The waves and currents have shaped its reef, shore platform, and beaches, fitting it well in all respects for its place by means of the forces that were to assail it; and an air of placid repose, as it lies amid the breakers, is its most impressive feature. Through the wind-made and tidal movements, the loose sands are drifted along the shores and over the reef; edges of the reef are broken off in gales or by earthquake waves; and occasionally a mushroom islet in the lagoon, where growing corals are not compacted by wave-action, is overthrown by the same means; but beyond this the structure is singularly defiant of the encroaching waters. Earthquakes may bring devastation; and so they may to other lands.

* Address, p. 23.

V. *Conclusion.*

With the theory of abrasion and solution incompetent, all the hypotheses of objectors to Darwin's theory are alike weak; for all have made these processes their chief reliance, whether appealing to a calcareous, or volcanic, or mountain-peak basement for the structure. The subsidence which the Darwinian theory requires has not been opposed by the mention of any fact at variance with it, nor by setting aside Darwin's arguments in its favor; and it has found new support in the facts from the Challenger's soundings off Tahiti that had been put in array against it, and strong corroboration in the facts from the West Indies.

Darwin's theory therefore remains as the theory that accounts for the origin of coral reefs and islands.

VI. *Central-Pacific Subsidence.*

Darwin, as has been said, took a step beyond direct observation in his inference that the subsidence attested to by each atoll extended over the intermediate seas and characterized a large central area of the ocean. He may be wrong here (and the writer with him) while not wrong in his theory. But, considering the distribution of the Pacific atolls in the ocean, their relation in this respect to the distribution of other Pacific lands, and the facts connected with the history of coral reefs and islands, the generalization appears to be well sustained. The question is here left without further argument, to be considered over the best geographical map of the ocean to be had, and the best bathymetrical map that can be made, only asking that the doubts which physical theory has set afloat may not be allowed by the geologist to warp the judgment or cripple investigation.*

My own agreement with Darwin as to the area of Coral-reef subsidence was promoted by an early personal study of the oceanic lands. For more than five years previous to passing my third decade I was ranging over the oceans—receiving im-

* One point often encountering an *a-priori* doubt is the slowness of the required subsidence. The subsidence over the Appalachian region which preceded the making of the Appalachian mountains amounted, according to well ascertained facts (as stated by Hall and Lesley), to at least 30,000 feet. The great trough, nearly a thousand miles long, was in progress through all of Paleozoic time. If the Paleozoic ages covered only 20,000,000 years (a low estimate) the mean annual rate was 0·018 inch, which is less than half a millimeter per year. Such a fact is no evidence as to the rate of the atoll-making subsidence; but, whatever the cause to which the Appalachian subsidence is to be attributed, it is suggestive as to possibilities and probabilities connected with the earth's movements.

pressions from a survey of the earth's features. I was made to see a system of arrangement in the Pacific islands, instead of a "labyrinth;" to appreciate the vast length of the island chains in the great ocean with their many parallelisms, and the accordant relations subsisting between them and long lines of atolls. I was thence led to observe the corresponding system in the features of the continental lands, and the more fully so when afterwards it was proved that geology was not in America merely the study of strata and fossils, but of the successive stages in a growing continent. Thus a conception of the earth as a unit became early implanted, and the idea also of its development as a unit under movements as comprehensive as the system in its feature-lines. My faith in any mountain-making theory hitherto proposed is weak. But that idea of system in structure and progress stands, and, however much ignored by students of the earth's stratigraphy, it must have its explanation in a true theory of the earth's dynamics.

ART. XXV.—*On a quartz-twin from Albermarle County, Virginia;* by W. G. BROWN.

IN 1851 G. Rose* described a group of quartz crystals from Reichenstein in Silesia, and announced that he had observed among them a new twinning law which he states as follows:

"Die Zwillingsebene ist nämlich, eine Hauptrhomboëderfläche; die Krystalle sind aber nicht mit dieser, sondern mit einer darauf senkrechten Fläche verbunden, und die Krystallgruppe besteht auch nicht aus zwei, sondern aus vier Individuen, indem an einen mittleren Krystall drei Individuen so angewachsen sind, dass eine Hauptrhomboëderfläche von jedem der letzteren mit einer der drei Hauptrhomboëderflächen des mittleren Krystalls in gleicher Ebene liegt."

In a note to this paper he mentions a specimen from the same place in the possession of Weiss, of which the transparent quartz crystals possessed a slight amethystine color; a color which seems to be common with the other occurrences to be mentioned. J. D. Dana† calls this group of Rose "a three-rayed twin, consisting of a central crystal twinned to three others by each R of one extremity," and gives the figures (figs. 198 A. B.) copied from Rose's paper.

Eck‡ took however a different view of the nature of this group and showed that it was not a twin according to the law

* Poggen. Annalen, vol. lxxxiii, p. 461, 1851.
† A system of Mineralogy, 5th Ed., p. 161, 1869.
‡ Zeitschr. d. deutsch. geol. Gesellschaft, vol. xviii, p. 428, 1866.

composition face R. Indeed he supposes that it was formed by the growth of quartz crystals upon the $-\frac{1}{2}R$ faces of calcite in a certain definite manner which he explains.

A little later Jenzsch* in a paper on the twinning laws of quartz, under the second law, which he calls the "Reichensteiner" or G. Rose's, seems to admit the explanation of Eck as applied to the whole group described by Rose, but says that for each two crystals in twinned positions G. Rose's law holds good, and gives an ideal figure representing two crystals twinned according to the law, twinning plane R. This figure (197 B) is in Dana.† Jenzsch seems to consider the statement of Naumann‡ that the twinning plane in these crystals instead of R may be $-\frac{5}{8}R$ "R (oder auch $-\frac{5}{8}R$)" as a quibble or attempt at a complete bibliography which it was hardly worth noticing.

That the view of Eck was the correct one is strengthened by the paper of vom Rath and Frenzel§ and that of E. S. Dana,‖ in both of which regular growths of quartz on calcite are described which, while not exactly agreeing with the groups described by Rose, seem to leave no doubt but that groups of quartz, "pseudo-twins," are formed by regular growth upon the rhombohedron $-\frac{1}{2}R$ of calcite. In the paper of vom Rath and Frenzel it is said that the faint reddish color of the Reichenstein quartz which reminds one of pale amethyst is due to disseminated particles of hematite. They further redescribe the group of G. Rose and give an exact drawing of the crystals (naturtreue Zeichnung).

Hare¶ says the form described by Rose is not excessively rare at Reichenstein and mentions, as do both vom Rath and Frenzel and E. S. Dana, that Breithaupt first noticed the regular growth of quartz on calcite and gives full references to Breithaupt's papers.

Since Jenzsch** says "Obgleich nun das G. Rose'sche Gesetz mit Evidenz bis jetzt nur von Reichenstein in Schlesien bekannt ist," and in the last edition (11th) of Naumann-Zirkel the view of Eck with regard to this kind of twinning is accepted, it appears justifiable to describe a group of amethystine quartz crystals found near Mechums River, Albemarle County, Virginia, in which there is twinning according to the law. Twinning plane and composition face $-\frac{5}{8}R$.

The group, which is transparent and of decidedly amethystine color is represented in nearly actual size by the accompanying figure. The sub-individuals marked *a* and *c* in par-

* Poggen. Annalen, vol. cxxx, p. 600, 1867. † Loc. cit.
‡ Elemente der Mineralogie, 6. Ed., p. 190, 1864.
§ Poggen. Annalen, vol. xvii. p. 155. 1875.
‖ American Jour. of Science, III, vol. xii, 448, 1876, and Zeitschr. für Krystall., vol. i. p. 39, 1877.
¶ Zeitschr. für Krystall., vol. iv, p. 244, 1880. ** Loc. cit.

allel juxtaposition have the face opposite *a*, over the apex of the crystal, in common, along with a third sub-individual in part shown in the figure on the lower left hand side of *a*. These three sub-individuals have also the ∞R plane parallel to that under *a*, seen in the figure in common. The second individual *b* is also composed of three sub-individuals in juxtaposition with the principal axes parallel. Using *a* to indicate one individual and *b* the other, it is seen that *a* and *b* are in a twinned position. The faces R of both individuals which are perpendicular to the plane of the paper give a single reflection and the faces of the sub-individuals *a* and *b* lie nearly in the same plane, not parallel planes which is the general case and which is represented by the faces R of *c* and *b*. The faces R which lie nearly in one plane, indeed all the well developed faces in the group, are slightly curved and striated so that a sharp reflection cannot be obtained for the purpose of exact measurement. Such being the case the following details are given.

The faces R are not *absolutely* in one plane, careful examination showing that the faces are inclined at an angle a little less than 180°. Further, when one face is horizontal the other is very slightly twisted around an axis joining the apices of the two crystals. Moreover the faces R and $-$R of the two individuals are not *exactly* in a zone, or what is the same thing the planes normal to R containing the principal axis in each individual would form a very small angle at their intersection. So nearly do the individuals approach the theoretical conditions that these points are mentioned to avoid giving the impression that the group possesses ideal symmetry.

The two conditions are practically fulfilled:

1. The faces R parallel to the same plane.
2. Principal axes contained in a plane perpendicular to R.

Under these conditions the angle between the principal axes would be 103° 34′ (the supplement of the angle between R and $-$R over the apex) and the twinning plane perpendicular to R, which is $-\frac{5}{8}$R.

Supposing $-\frac{5}{8}$R to be the twinning plane and calculating backwards it is found the faces R, of the two crystals, would form an angle at their common intersection of 179° 34′ an angle a little less than 180° which is the case with the group here described, and that the inclination of the principal axes of the two crystals would be 104°.

It may be stated that the lower portion of the group, represented in the figure by irregular lines, consists mostly of planes having a definite orientation to the dominant individuals.

The group was found loose in the soil and there are no signs of its having been broken off another piece or that there have been any crystals attached after the manner of those in the groups from Reichenstein.

The only planes that can be definitely recognized are R, −R, ∞R. The faces R and −R possess essentially the same luster, so that there is no physical means of distinguishing the one or the other.

The foregoing article was prepared while the writer held one of the Morgan Fellowships in Harvard College, and this opportunity is taken to express the gratitude he feels to the Director of the Boylston Laboratory, Professor J. P. Cooke, for the unvarying kindness with which Professor Cooke gave aid, counsel and encouragement.

University of Virginia, May 12th, 1885.

Art. XXVI.—*Impact Friction and Faulting;* by Geo. F. Becker.

(Continued from page 128.)

The first portion of this article was devoted to a discussion of the distribution of energy in a rod of finite or infinite length, and of elastic or inelastic material, when subjected to an impact in the direction of its axis. It was also pointed out that such a rod might be divided into short portions supposed to remain in contact with one another without affecting the result. It was then shown, that if for such short rods, sheets or plates lying one upon another are substituted, each plate being constrained to move in its own plane, but presenting on its upper and lower surfaces an indefinite number of extremely small inequalities or teeth interlocking with the similar inequalities of the adjacent plates, the distribution of energy through the system of sheets caused by the impact of these teeth when the uppermost sheet is moved is the same as in a rod subjected to impact except that the axis of distribution is vertical to the line of motion instead of coincident with it. A surface armed with such minute inequalities or teeth, is the conception of a frictional surface temporarily adopted.

Application of principles of impact to friction.—Considering friction as a form of impact under particular geometrical conditions, it is easy to make applications of the results obtained for impact to frictional problems. Were the material of sheets offering frictional resistance perfectly elastic and without elastic limit, the movement of one plate over another would in general be impossible. The projections on the two surfaces would

interlock and any force applied would deform the two plates as a single elastic mass. It appears indeed not beyond the bounds of possibility, that the inequalities of a surface might be perfectly elastic but of such a form as to yield passage to a body passing over them by bending and compression. Such a case would correspond to the impact of perfectly elastic bodies in which a portion of the energy is converted into vibrations; in other words to a case in which, in spite of perfect elasticity, the coefficient of restitution is not unity. Such cases, if they exist at all, must be very rare; for it is a matter of daily experience that highly tempered steel-cutting instruments are sharpened upon, and are therefore abraded by, friction upon soft and greasy leather. If on the other hand the material were inelastic, or partially elastic, work would be done on the contact and relative motion would take place. This relative movement would be greatest for total inelasticity. Partially elastic sheets would not separate like partially elastic balls, but would move relatively by an amount corresponding to the work done in members of a system of partially elastic balls. The material remaining the same and the coefficient of restitution constant, the number of minute projections on any pair of contact surfaces would make no difference in the work done at that contact though it would determine the ultimate relative positions of the centers of inertia of two sheets. The tiny projections of material surfaces are certainly not prisms with vertical sides, but irregular in shape and larger at their bases than at their summits. The closer surfaces are brought together therefore, the more such projections will interlock and the greater will be the expenditure of energy requisite to grind them off.

Corresponding interpretation of Morin's laws.—The results obtained by Morin from his own and Coulomb's experiments on friction are expressed in rules known to be approximately and only approximately true. They are that friction is proportional to the area of the rubbing surfaces, proportional to the pressure, and independent of the velocity. The first relation is evident, the second states that the interlocking of projections increases with the pressure, as it certainly must, and furthermore that this interlocking is such that the amount of work to be done increases in simple, direct ratio to the pressure; this is only approximately true. The third relation, that the amount of work done is independent of the velocity, manifestly implies the assertion that the coefficient of restitution is independent of the velocity, or that it is constant for partially elastic bodies and beyond the limit of elasticity as well as under conditions where no sensible deformation occurs. It was of course never claimed that this rule was exact or that it was applicable

except for moderate differences of velocity. A great advance towards the elucidation of the true relation between friction and velocity might evidently be made by a study of the relation between the coefficient of restitution and the velocity of imperfectly elastic impinging spheres or cylinders.

Effect of extreme velocities.—If there is relative translation between rubbing masses there must always be more or less "play," or a slight possible movement normal to the frictional contact, so that the centers of inertia of such masses cannot be confined to motion in absolutely straight lines. This is of importance in the extreme cases of zero velocity and very great velocity. The coefficient of friction for rest appears from experiments to be merely the maximum value of the coefficient for motion, and its excess is almost beyond a doubt due in part to the fact that, when at rest under a given pressure, the projections of the opposing surfaces interlock more deeply than they can under the same pressure when in motion. In the case of high velocities the mean distance of the centers of inertia of rubbing masses must be greater than for low velocities, owing to the increase of the elastic rebound, resolved perpendicularly to the contact plane, when the more or less irregular inequalities impinge.* These will therefore interlock to a smaller extent, and the friction will diminish from this cause independently of any variation of the coefficient of restitution. This coefficient also stands in some not precisely known inverse relation to the velocity so that friction should be expected to be notably less for very high velocities than for moderate ones as experiments have shown is the case.

Results for a system of sheets.—If then a system of material sheets is arranged say in a vertical pile, there being perfect contact throughout, and impulsive energy is imparted to the uppermost of them, this energy according to the deductions drawn will be distributed throughout the system exactly as in a series of partially elastic spheres. Assuming that the coefficient of restitution is constant and not unity, every sheet which receives energy must move and must impart kinetic energy to the next following. The distribution of energy must be logarithmic (simply or compositely) quite irrespective of any differences of frictional resistance which may exist at different contacts. On the other hand if the friction at all contacts is the same, the movements of the sheets will be logarithmic and if the friction varies according to any law, this combined with the law of the expenditure of energy will give the locus. Morin's law that

* Imagine for example a mass of india-rubber dragged over a street pavement; the elastic mass would constantly rebound from the irregular paving stones, and the more rapid the translation the higher would be the mean position of the center of inertia of the rubber mass. Morin shows that the resistance offered by pavement to a vehicle provided with springs is less than to one without springs.

friction is independent of velocity not being rigidly correct however, if two partially elastic sheets are in contact one of them may as a matter of fact receive an impulse so slight that the elastic resistance of the interlocking projections will not be overcome. An elastic strain but no motion on the contact plane will be the consequence.

Independent kinetic theory of system of sheets.—While there thus seems every reason to suppose that friction is the resistance due to the impact of the minute projections which distinguish material surfaces from mathematical planes, the kinetic theory of a system of sheets is not dependent upon it, but merely upon the existence of some resistance which allows of relative motion yet converts "molar" energy into molecular energy. Whatever may be the nature of this resistance it cannot conflict with the principle of the stability of the center of inertia of a system upon which no external forces act and it is a necessary conclusion that, if no obstacle exists to intercept the transmission of energy, the motion of the first of an infinite series of sheets necessitates the motion of all. If these sheets are infinitely thin as well as infinite in number, the diminution of the energy between two successive surfaces, or dw can be neither more or less than wdx, and the distribution of energy is simply logarithmic. The argument may be extended to the case of a finite system of sheets exactly as the behavior of a finite rod under compression was inferred from that of an infinite rod. In short

$$\frac{c^2 d^2 w}{dx^2} = w.$$

But if the results derived from the consideration of friction as mere resistance of unknown character coincide mathematically with those deduced from considering friction as a form of impact, this conclusion is certainly strongly confirmatory of the physical hypothesis.

Elastic mass under shearing strain.—Exactly as a series of inelastic cylinders in contact (or an inelastic rod) and a similar series of elastic cylinders accurately coincide in behavior during the period which elapses between the first contact of an impinging mass and the moment of maximum compression, so also does the behavior of a series of sheets in contact represent the behavior of a solid completely elastic mass. In other words precisely as the energy actually expended in the deformation of inelastic substances during an impact may be conceived as potentialized, so also may the energy expended in friction be treated as potentialized. This statement which is almost self-evident has ample authority.* If an elastic rod of uni-

* Thomson & Tait, Nat. Phil., 1st ed., § 452.

form cross section with its axis placed parallel to X were artificially prevented from any deformation in planes parallel to its terminal surfaces and one of these surfaces were strained by a suitable stress into a position in which its central point no longer coincided with the axis, the same differential equation would express the distribution of energy. This result follows quite independently of any analogy, but the steps necessary to show it are a mere repetition of those given in detail for a compressive strain. Now imagine such an elastic rod thus strained to be suddenly intersected by any number of planes parallel to its ends. If these surfaces presented no frictional resistance the centers of inertia of the intervening sheets of elastic material will have no tendency to move and they will remain on the simple or composite logarithmic locus. Or suppose them to present a frictional resistance independent of the velocity. At the instant when the partings are formed the system is in equilibrium and the center of inertia of each sheet may be regarded as held in check by the forces exerted upon it by its two neighbors. Two suppositions may then be made: the frictional resistance may be conceived either as that presented by inelastic substances or as that presented by partially elastic substances. (If the interlocking particles were wholly elastic no motion would take place). In the former case no energy could be transmitted from one sheet to the next and the whole energy potentialized between the planes of the centers of inertia of two adjoining sheets would be expended upon the contact between them and the curve of the centers would coincide with that found when the surfaces are frictionless. If the resistance is partially elastic, e remaining constant, a certain proportion of the energy potentialized would be expended at each contact and the locus of the centers of inertia would vary from the preceding only in the value of the constants.

Coefficient of friction—If, as in the experiments to be described presently, a rigid mass W moves over a system of sheets with a fixed quantity of energy, it will move precisely as far as if the upper sheet of the system were fixed and no energy were communicated to the remainder of the system. If A is the distance which W moves and f is the frictional resistance per linear unit, the energy expended in friction is Af. If the mass of W is M and its initial velocity v and were the system of sheets totally inelastic, Af would equal $\frac{Mv^2}{2}$. Were the sheets wholly elastic (or more exactly wholly restitutional), no energy would be expended in friction and there would be no permanent deformation. (The whole energy would be first converted into "molar" vibrations, and in any actual case

would pass over by friction against the atmosphere, internal friction, etc., into molecular energy). If the system is partially restitutional it is clear that

$$f \cdot \mathrm{A} = \frac{\mathrm{M}v^2}{2}(1-e^2),$$

where e is the coefficient of restitution for velocity and $(1-e^2)$ the coefficient of expended energy. If k is the coefficient of friction and the normal pressure is $\mathrm{M}g$ (where g is the acceleration of gravitation if the pressure in the system is due to the weight of the mass W, or a corresponding velocity if the pressure is otherwise induced),

$$f = k\,\mathrm{M}g,$$

and if $\mathrm{A}=1$

$$k = v^2\frac{1-e^2}{2g},$$

so that the coefficient of friction may be reduced to two determinable velocities and the coefficient of restitution. Here however the extent to which the inequalities of the frictional surfaces overlap is supposed constant, or the elasticity of the rubbing masses considered as represented by their centers of inertia, is not taken into account, nor shall I attempt to discuss it.

The equation of the distribution of energy in an infinite system of sheets will then be

$$w = \frac{\mathrm{M}v^2}{2} \cdot \frac{1-e^2}{c} \cdot \varepsilon^{-x/c},$$

c being the subtangent. If the coefficient of friction is uniform throughout the system, the differences of the movements of successive sheets will be in precisely the same ratio as the absolute movement of the sheets, so that if y is the distance moved by any sheet and μ the ratio of the movement of adjoining sheets,

$$y = \mathrm{A}\varepsilon^{-x/c} = \mathrm{A}\mu^{-x}.$$

For this case therefore

$$\frac{f}{c} = \frac{w}{y} = \frac{\mathrm{M}v^2}{2}\frac{1-e^2}{\mathrm{A}c}.$$

The frictional resistance being uniform its actual value will affect the zero value of w, while μ and c are entirely independent of f, or, if the subscript indices 1 and 2 are used to indicate successive sheets in any portion of the system,

$$\mu = \varepsilon^{1/c} = \frac{y_1}{y_2} = \frac{\Delta y_1}{\Delta y_2} = \frac{f\Delta y_1}{f\Delta y_2} = \frac{w_1}{w_2},$$

and f appearing both in the numerator and denominator does not affect μ so long as f is constant. On the other hand if

f increases with x (as it really does, though slowly) μ and c are functions of f. If f is not uniform, the strain curve will be the result of the combination of the logarithmic distribution of energy with the varying coefficient of friction and will not therefore in general be simply logarithmic. But were the relation of f to x logarithmic, so that $f_2=\nu f$, ν being a constant

$$\mu=\frac{f_1\Delta y_1}{\nu f_1\Delta y_2}, \text{ or } \mu\nu=\frac{\Delta y_1}{\Delta y_2}.$$

It readily follows that y_1 is to y_2 in the same ratio for this case, and therefore the strain equation would be

$$y=\mathrm{A}\,(\mu\nu)^{-x/c}.$$

Now according to Bochet the locus representing the relation of the coefficient of friction to velocity is represented approximately by the equilateral hyperbola referred to axes parallel to the asymptote.*

For a series of velocities standing to one another in a constant ratio and for a uniform total pressure at all contacts, the ratio of successive values of the coefficient of friction will therefore be nearly constant, or in other words ν will vary very slowly for the range of velocities for which Bochet's formula fairly represents the facts, and the locus of the edges of the sheets will vary but little from a simple logarithmic curve. It will be shown later that while this is true for very moderate velocities, it is possible to experiment with velocities so low as to reveal the variation of ν. If f varies at different contacts, of course the ratio of successive values of w is not the same as the ratio of successive values of y.

Coefficient of restitution—If the strain is proportional to the stress in a system of uniform sheets the strain equation may be written

$$y=\mathrm{A}\varepsilon^{-x/c}=\mathrm{A}\mu^{-x},$$

where μ is the ratio of the movement of two successive sheets. The quantity c is therefore the length of the subtangent expressed in terms of the thickness of a sheet. If c is taken as unity (making $\mu=\varepsilon$) and if the coefficient of restitution is zero

$$\frac{dw}{dx}=-w.$$

*Ann. des Mines, 1861. If k_0 is the statical coefficient, and $k\infty$ the value of k for an infinite velocity and v the velocity in metres per second, Bochet's formula may be written

$$k=\frac{k_0-k\infty}{1+0.3v}+k\infty.$$

To illustrate the statement made in the text, let k_0 be 0·5 and $k\infty$ be 0·1. If the value of k is computed for $v=\frac{1}{16}, \frac{1}{8}, \frac{1}{4}, \frac{1}{2}, 1, 2, 4, 8, 16, 32, 64$, and each of these values is divided by the next following, the ratios or values of ν obtained are in the same order 1·01, 1·03, 1·06, 1·09, 1·17, 1·25, 1·27, 1·29, 1·23, 1·17.

If this last case is compared with another in which the coefficient of restitution is not zero, w' for the same value of w will evidently be smaller than the above; and, by a similar argument to that adopted in determining the total expenditure of energy, it follows that

$$\frac{dw}{dx} = -(1-e^2)\,w.$$

Here then the subtangent instead of being unity is

$$c = \frac{W}{W'} = \frac{1}{1-e^2},$$

so that c is a function of e which can become unity only when $e=0$. The unit of measurement in this equation is the value which the subtangent would have were $e=0$ or the system wholly irrestitutional, but it is also the length of the subtangent in terms of the thickness of a sheet and consequently if the system is irrestitutional the length of the subtangent is the thickness of a sheet, and in general if the thickness of a sheet is δ,

$$c\,(1-e^2) = \delta.$$

Now from an experimental case it is extremely easy to determine c and δ so that the coefficient of restitution may readily be found for a given case from the equation

$$e = \sqrt{1-\frac{\delta}{c}} = \sqrt{1-ln\mu}.$$

For $e=1$ or for a perfectly restitutional system $c=\infty$ and $\mu=\varepsilon^0=1$ while for $e=0$, $c=\delta$, or if δ is the unit, $\mu=\varepsilon$, and these are the limiting values of μ.

Friction in terms of elasticity—The energy equation for an infinite system of sheets may therefore be written

$$w = \frac{Mv^2}{2}(1-e^2)^2\varepsilon^{-x(1-e^2)}.$$

and if the functional relations of e and the velocity were known w could be immediately stated in terms of velocity. On the other hand, experiments on friction are capable of yielding values of e, though whether this would be convenient and expedient method of investigation is another matter. Be this as it may, the above equations establish a simple and rational relation between friction and restitution; they show that friction may be removed from the anomalous position it has hitherto occupied, and brought under the great problem of elasticity.

Geometrical differences between systems under friction and impact.—There is a formal difference between the behavior of a finite system of sheets resting against a rigid obstacle and that of a

finite compressible rod. In the latter case I have shown that the energy of an impact is to be regarded as divided into two portions acting from opposite directions upon the passive mass and that the energy curve is a projected catenary, while if the strain is proportionate to the stress, the geometrical result is of the same character. In the same way, if a finite system of sheets rests on a rigid mass and the topmost sheet is moved against friction by an impulsive application of force, this force is to be regarded as decomposed into two shearing stresses acting on the upper and under surfaces respectively in opposite directions. The energy curve is the same as for a rivet, but the geometrical result differs because, as the rivet may be conceived as at first forming part of an infinite rod and then as receiving the energy which would be distributed over the whole mass beyond the point $x=a$ from the opposite direction, so a finite system of sheets is to be regarded as at first of infinite thickness and arranging itself on the simple logarithmic curve and then as receiving the energy potentialized beyond $x=a$ in a direction parallel to that of the original impulse, but differing from it by an angle of 180°. If the strain is proportionate to the stress, the geometrical result will clearly be a curve of the form

$$y = \mathrm{A}\,(\varepsilon^{-x/c} - \varepsilon^{x/c}),$$

the origin being taken at the point of inflection which will be on the same ordinate as the horizontal point of the energy curve.

There is a corresponding difference between the results of the impact of two compressible masses and the mutual action of two systems of sheets, if the two systems move relatively on the only plane common to both. In each case the energy curves of the two systems will meet in a salient point on a line corresponding to the contact plane. For the impinging compressible masses the geometrical result will be of the same character as the distribution of energy. The two systems of sheets on the other hand will arrange themselves on two (simple or composite) logarithmic curves with opposite curvature, so that if the material of the two systems is the same the junction will be a point of inflection instead of a salient point.*

A similar reversal takes place in certain cases of what may be called negative impact. If an iron rod is brought in contact with the surface of a mass of melted glass and withdrawn, the thread obtained will be logarithmic (for evidently dw will equal minus wdx). If instead of an iron rod a glass rod is substituted, and this is allowed to remain in proximity to the heated mass long enough to become somewhat softened, the hot end of the glass rod will be drawn down by the resistance

* The subject is treated at length and illustrated in Geology of the Comstock Lode, p. 173, *et seq.*

of the weight depending from it and the curve shown by a longitudinal section of the rod will be continuous with that of the logarithmic column, instead of showing a salient point at the junction as would be the case in a corresponding positive impact. Similar principles apply under the slightly more complex conditions existing when a glass rod is softened in the middle before a lamp and drawn out from the cold ends.

Experiments on friction.—More or less exact experiments can readily be made to test the statements as to friction here made. If a flat bar (such as a standard steel foot rule) be laid upon a frictional surface (blotting paper for instance), and a yielding mass (say a rubber pencil eraser) be drawn along its upper surface, it will be found that a slight pressure suffices to set the bar in motion and that there is simultaneous relative motion between the rubber and the bar and between the bar and the blotter. If a board about as thick as the bar is broad is covered with thick paper of the same kind as that on which the bar rests, and the edge (thus prepared for the sake of a better contact) is pressed upon the bar and moved in the direction of its longer dimension, relative motion on each side of the bar will again take place. In these experiments if the pressure applied is very light, no relative movement will occur below the bar because a certain amount of energy is needful to strain the minute projections of the paper beyond their elastic limit. These trials show that motion is really produced by friction if under that term the whole phenomenon is understood, or concomitantly with friction if the word is limited merely to the dissipation of energy accompanying the action. Still further to test the character of this transmission of energy, I arranged a pile of seven steel bars one foot long $1\frac{1}{4}''$ wide and $\frac{1}{12}''$ thick, inserting between each a slip of blotting paper of the same width and length as the bars. These slips of paper were considered necessary to effect fairly complete contact which could have been attained otherwise only by grinding the bars together with polishing powder. To adjust the strips of blotting paper to the steel surfaces and to remove grosser inequalities in the paper, each bar was rubbed a few times backward and forward on the underlying slip. A long strip of wood was then laid upon the strip of paper covering the topmost bar and this was uniformly weighted over the bar with some ten pounds of lead. The wooden strip was then drawn away by a horizontal traction for three or four inches. The energy was distributed throughout the system, relative motion taking place from the first instant on 13 out of the entire 15 contacts and the ends of the bars were manifestly arranged on a curve asymptotic to their original plane and almost certainly recognizable as the logarithmic curve to an eye familiar with its form.

Still more satisfactory is an experiment with slips of paper.* If a considerable number of slips of uniform paper are arranged in a pile held down by a weight, and a blunt edge, such as that of a ruler, pressed upon them near the weight and drawn forward at right angles to the direction of the edge the fraction of an inch, the edges of the sheets will be found to occupy a curve. If the operation is repeated a number of times the curvature becomes very sensible. This curve can be plotted from the experiment with great ease and will be found to coincide most remarkably both as to curvature and the position of the asymptote with the logarithmic curve.

If the sheets were perfectly flexible the pressure throughout the system under the narrow area in contact with the ruler would be confined to nearly the same area at each contact. The weight of the slips of paper, it is true, causes an increment in pressure from the surface downward, but this is inconsiderable as compared with the combined pressure of weight and ruler. If the slips are not perfectly flexible, the pressure is spread over a wider and wider area from the top downward. This however makes no difference in the result, since the frictional resistance varies directly both as the surfaces under pressure and the intensity of the pressure per unit of area, in other words, as the total pressure which is necessarily the same (neglecting the weight of the slips) at each contact, however the pressure may be disseminated. For a single edge many edges or points may be substituted. If for instance a few dozen grains of sand are strewn over the upper sheet and a bit of plank be pressed upon the system and moved, the sheets will still arrange themselves on the same curve. Even a single point (a pencil point) will bring about the same result. On the other hand, if the weight is uniformly distributed over the whole upper surface and drawn away, the system will break at one or a few contacts only. The experiments indicate what might have been supposed *à priori* the reason of this failure. Films of air occupy the by no means infinitesimal intervals between the sheets and, unless these are broken by localized pressure, such as is produced by a point, edge or narrow weight, these films are shifted irregularly and prevent the uniform action of friction. The phenomena are then comparable rather with those of lubricated surfaces rather than with dry friction. An almost self-evident modification of the experiment confirmatory of this explanation may be effected by setting the blunt-edged instrument in the line of motion instead of at right angles to it, so that the pressure is uniform from end to end of the sheets beneath the edge. If the edge slips too easily, or is too smooth to communicate the necessary amount of energy to the

* Described at length in Geol. Comstock Lode, p. 165.

system, sufficient friction can be induced by fastening round it paper of the same kind used for the sheets. The resulting curve is the same as that produced by applying the edge at right angles to the direction of motion.

In experiments with slips of paper it is easy and desirable to apply the pressure and draught mechanically, although an excellent coincidence with the logarithmic curve can be obtained with a ruler merely held in the hand. If in mechanically conducted experiments the velocity is about that which one would naturally employ in a trial by hand, say six or eight inches per second, the coincidence with the logarithmic curve is almost perfect.* If, however, by a very light pressure and slow mechanical movement with an edge or soft blunt point the uppermost sheet is given an extremely slow motion so that the rate of movement of say the tenth sheet is scarcely perceptible, the curve produced has a subtangent which increases rapidly from the first sheet onward. So also must therefore the quantity I have called ν and with it the coefficient of kinetic friction for these extremely low velocities, or as the value of the statical coefficient is approached. Judging from this indication it would appear that a good empirical approximation to the relation of k to v might be obtained by adopting the tractory (or "anti-friction" curve $k=c\,\epsilon^{-s/c}$) rather than the hyperbola to represent experimental results, but the value of k in terms of v might then be too complex for practical use. The value of the coefficient of restitution indicated by experiments on paper is as it should be, very high. In my former paper I noted a case in which μ was 1·4. This answers to $e=·81$. With other qualities of paper I have obtained values of μ indicating a restitution of considerably above 90 per cent.

Conclusions reached.—In the foregoing pages the attempt has been made to approach the subject of friction as a problem in elasticity. I began by discussing the distribution of energy in an elastic compressible rod and showed that this is represented in general by the simple equation $w = c^2 w''$. Applications of this equation to some special cases were made and some of the modifications necessary for incompressible substances were pointed out. By regarding friction as the impact of the minute inequalities of material surfaces, a passage was effected from impact to friction and from a compressive to a shearing strain. The significance of Morin's laws under this supposition was enlarged upon. It was then shown that the same conclusions followed independently of the physical hypothesis adopted, a fact regarded as confirmatory of this hypothesis. It was at last

* See Geol. Comstock Lode, p. 167, fig. 4.

found that the formulas for the distribution of energy in a system of sheets could be reduced to terms of the total energy and the coefficient of restitution which brings friction under the head of the general elastic problem and accomplishes a main purpose of the inquiry. Various experiments designed to test the analytical results were then described. It only remains to draw some general conclusions as to the character of friction and to point out the application of the results to the geological problems which led to the investigation.

Character of Friction.—The term friction seems to be somewhat variously applied in literature, sometimes indicating the whole resistance offered by a body to the passage of another over its surface and sometimes only the surface resistance. It appears to me more convenient to restrict it to the latter sense, because in this case it is measured by what is known as the coefficient of friction. When one body tends to move over another, the contact being frictional, the resistance of the passive mass, as in any other case of motion, is the resistance which it offers to acceleration, or its reaction against the active force. The active force in this case is a shearing stress and its effect is a shearing strain which is distributed throughout the entire passive mass. The resistance as a whole, therefore, is a counter shearing stress, while friction considered as a surface resistance is the value of this counter-stress for a particular plane. If the passive mass is nowhere strained beyond its elastic limit no relative motion can take place under ordinary circumstances and the electric resistance of the minute inequalities of the frictional surface is what is known as statical friction. If the stress is sufficient to strain the minute inequalities beyond their elastic limit, or to bend and compress them out of the way, relative motion takes place. The conceivable case of surfaces wholly elastic but only partially restitutional, which would allow of relative motion, does not seem to occur, though in some instances it is approached. The imperfectly restitutional resistance of the minute projections is kinetic friction, and as a matter of experience is always attended by permanent deformation. It must in all cases be accompanied by a strain of the entire passive mass, though this strain may be elastic and commonly is so. But if the passive mass is itself a system presenting planes in which resistance is inelastic, motion and permanent deformation must occur on them also. The supposed entire inelasticity, like perfect elasticity, is ideal. The fact that any surfaces, however inelastic, produce sound when undergoing friction shows that the material is capable of molar vibrations. It is fair to conclude that the coefficient of restitution also never reaches its limiting value, unity, which is of course also inferable from experiments on impact. If the resistance on the planes

above referred to is partially elastic, motion and permanent deformation would always occur on them also were the coefficient of restitution and therefore also the coefficient of friction independent of velocity. Motion and permanent deformation will actually occur on such planes unless the strain falls short of the elastic limit or, practically unless the strain is extremely slight. The frictional resistance depends upon the geometrical form and absolute dimensions of the minute projections on the rubbing surfaces, the extent to which they interlock, and upon their coefficient of restitution. To a certain extent surfaces rubbing will polish one another and friction will progressively diminish, but the dust resulting from attrition will always suffice to produce new inequalities, while the smoother the surfaces become the greater must be the influence of the welding which according to Messrs. Thomson and Tait occurs even in the mere impact of polished spheres. A tearing of the surface must follow cohesion at any point, so that this cause also must continually interfere with the reduction of the surfaces beyond a certain limit of smoothness. The character of the surface of minimum friction resulting from the action of friction itself is probably open to investigation. It will evidently be closely connected with several of the physical properties of the material. In any inquiry into friction it should not be forgotten that the extent to which the inequalities overlap is in part determined by the elasticity of the whole masses in contact, which must therefore be taken into consideration as well as the coefficient of restitution of the inequalities of the surfaces.

Geological application.—Suppose a mass of rock in place is divided by any means into parallel sheets: let the country adjoining one side of this system of sheets be solid; and let the whole system be held together by a pressure normal to the fissure planes. If the solid mass adjoining the system of sheets is forced to rise, the conditions discussed at such length in the foregoing pages are present. The energy of the moving mass will be distributed through the system logarithmically. If the system is infinite, or contains a considerable number of sheets, and if the coefficient of restitution is constant, the distribution of energy will be represented by the simple logarithmic curve however the friction may vary. If the friction is the same on all the contacts, the geometrical result will be represented by the same curve. If the friction remaining constant the system is regarded as including a finite number of members the geometrical results will be represented by the difference of two simple logarithmic curves. These are the three fundamental propositions asserted, and so far as I can see rigidly proved in my former paper on faulting, but reached by a wholly different method from that here adopted. New experiments as well as

new proofs are given above and the analysis is further extended. That a fault under a compressive stress in a mass of rock cut by a single fissure will tend to produce a system of parallel fissures, I showed in my former paper; I have nothing to add on that subject. This fact explains the formation of parallel veins, the parallel fissure of step-faults and the sheeted structure of landslides which are merely superficial faults.

Faults are known to be in a large proportion of cases the result of a tendency to fold carried beyond the limit of elasticity of the rock. The simplest case of faulting is therefore an anticlinal fault passing over into a fold at its extremities. I have shown that under certain conditions instead of a single fissure a system of parallel fissures will be formed, and that the movement will be distributed over all of them. The upper end of any sheet will then remain parallel to its original position. In a stratified country intersected by such a fissure system the result is, strictly speaking, a system of monoclinal faults. The divisions of the step-faults which I have personally inspected, however, are all too insignificant in thickness to be ordinarily or conveniently classified as monoclinal faults. That this is not always the case is apparent from Mr. Geikie's illustration* of a step-fault in the coal fields between Linlithgow loch and the Frith of Forth. The logarithmic character of this fault is manifest from the illustration, while, if I understand the geography aright, the separate benches must be as much as a quarter of a mile in width. Even if the section is not to scale, the width of the sheets must evidently be such that no one would hesitate to include the case among monoclinal faults. In this particular instance the strata are practically horizontal. This is a matter of no consequence however, for, had they been inclined previous to the intersection by the fissure system, they would still be inclined at the same angle. Thus it is certain that some monoclinal faults are explicable on the theories set forth above and their relation to anticlinal faults is at once evident. They result from the substitution of a system of fissures for a single fissure in an anticlinal fold followed by the distribution of motion over all the contacts. In all such cases it is unnecessary to suppose a tilting of the monoclinal blocks in a plastic magma after separation from the adjoining country, or to conclude that the fissures have gaped to allow of such tilting; for step-faults involve the compressive strains which most geological phenomena lead us to suppose the rule in geological dynamics. A series of blocks tilted after separation from the surrounding country also leave a triangular space at each end of the system which must apparently fill with the plastic magma, a complication which will not be met with in step-faults. This

* Textbook of Geology, p. 532.

fact affords a distinction which may in some cases serve to decide the character of a particular fault.

Since different lines of research lead to the principle of the logarithmic distribution of energy in systems of sheets connected by friction, and since this result is confirmed by experiments and observation it appears to me that it may fairly be claimed as established and as affording a valid explanation of the structural problems presented by step-faults, parallel systems of veins, and of some monoclinal faults and landslides.*

I shall take advantage of the first space which may be allowed me in this Journal, to show that the same principles apply to another and very different problem in structural geology, and to present an entirely fresh proof of the fundamental equation.

Office of the U. S. Geological Survey,
San Francisco, February, 1885.

* Mr. Ross E. Browne, of the University of California, has published an attack upon the theory of step-faults given in my report upon the Comstock Lode (this Journal, vol. xxviii, p. 348). I feel great diffidence in opposing my opinions on this subject to those of an author who writes in so authoritative a tone, for my own knowledge of the science of mechanics is rudimentary. I am nevertheless forced to the conclusion that Mr. Browne has failed to perceive the full significance of Morin's laws. To grant that friction is independent of velocity and yet to deny that there will be motion on each contact of a system of sheets, such as that discussed above is simply to deny the presence of a stress, for this independence implies movement (a strain) however slight the stress. But to deny a stress under such conditions would be to deny the principle of the equivalence of action and reaction. So, too, Mr. Browne disputes the applicability of my experiment on sheets of paper as a test of the theory proposed, because the pressure of a blunt edge will not be confined strictly to the same area at each contact. Yet this can make no difference under Morin's laws, since under them the frictional resistance at any contact depends upon the total pressure exerted upon the entire area of the frictional surface and not in the least upon its distribution. Morin's laws formed the basis of my analysis as was stated over and over again in my former paper, though I endeavored to experiment in such a way as to be as little dependent upon them as possible.

Rejecting my explanation of the curve obtained in the experiment Mr. Browne offers as an improvement one founded on what appears to me a strange physical hypothesis and which leads him to the hyperbola $y=c/x$. In my report, p. 167, I compared the experimental curve with this locus, and showed that the position of the asymptote is fatal to the supposition. If this is not regarded as a sufficient objection the incorrectness of the solution will be evident when it is considered that even if the equation is given the form $y=c(1+x)^{-1}$, $\sum_0^\infty (1+x)^{-1}=\infty$; so that the hyperbolic curve could only result from the application of an infinite amount of energy. In short, without going into further detail, I fail to see that Mr. Browne has pointed out any error in my work or that he has thrown any additional light on the subject.

Art. XXVII.—*Note on the Transmission of Light by Wire Gauze Screens;* by S. P. Langley.

In the beginning of the present year, a friend sent me a series of wire-gauze screens, which he used to diminish the apparent brightness of stars in making meridian observations, with a request that I would determine photometrically the amount of light transmitted by them. As such screens are occasionally employed in astronomical work, particularly in the use of the heliometer, I have thought the following account of our experience of sufficient interest to make public.

I used for the measurements a photometer box originally constructed for another purpose, and an opaque wheel or disc, having radial slits of variable width, which placed in the path of a ray of light and rotated with sufficient velocity, can be made to reduce the light to any desired fraction of its original intensity. (This I have employed for some years for photometric measurements when it is desirable to avoid the use of polarizing apparatus.) In the center of the photometer box was a sliding Bunsen disc which could be viewed from above by a suitable arrangement of mirrors. The open ends of the box were directed to two opposite windows, and the disc placed in such a position that its sides were equally illuminated. The wire screen was then placed over one end of the box, the wheel photometer in front of the other end, and the apertures of the latter altered until the equality of illumination of the Bunsen disc was restored. The screen then cut off the same amount of light as the wheel. From several series of measurements made in this way it was found that

1 screen transmitted	·395 ± ·004	of the incident light.
2 screens superposed transmitted	·144 ± ·004	" "
3 " " "	·052 ± ·003	" "

These numbers, as was to be expected, are nearly in geometrical progression. The screens were returned to the sender and the results communicated to him, but he wrote that upon trial, he found the reduction of light very much greater than the above values, three superposed screens reducing the light of a star by 7·1 magnitudes, which corresponds to a transmission of only ·0014.

I was at that time absent, and my assistant, Mr. J. E. Keeler, undertook the investigation of the cause of the discrepancy, which he attributed to loss of light by diffraction under the circumstances in which the screens were used by their owner, i. e. in front of the object-glass of a telescope directed upon a star. With diffuse light such as was used in the measurements with the photometer box, no loss due to this cause was possible.

In the apparatus devised by Mr. Keeler for an experimental determination of the loss by diffraction, the star was replaced by an illuminated pinhole in the focus of a 3-inch collimating telescope. This was viewed by an observing telescope of nearly equal size, in the eye-tube of which was an unsilvered plane-glass mirror, which reflected into the eye-piece a comparison star—the image of an illuminated pinhole produced by a collimating telescope at right angles to the other two. In the path of the rays from this telescope could be interposed the wheel photometer. The light before entering the first pinhole suffered reflection from an unsilvered glass surface in order to reduce its intensity to that of the comparison star.

The two images in the field of view having been adjusted to equality, the wire-gauze screen was interposed between the object glasses of the collimating and observing telescopes, reducing the light of the star and producing around it the well known diffraction image of a network. The wheel photometer was then introduced, and the intensity of the comparison star reduced until it was equal to the central image of the other. By enlarging the pinholes until the superposition of the colors produced white light, the intensity of the diffraction images could also be estimated.

It was thus found that the central image had only ·175 of its original brightness, which would therefore be the proportion transmitted by the screen under these conditions, and that the brightness of each of the four first spectra was ·05 of that originally possessed by the central image. Two thicknesses of the wire-gauze transmitted barely ·02 as measured by the intensity of the central image.

The screens with which these experiments were made were much coarser than the original ones, and it was expected that the effect of diffraction would be less pronounced. The transmission of one thickness, measured by the photometer box, was ·47, of two thicknesses ·21.

Finally the apertures of the screen and the diameter of the wire were measured by a micrometer microscope and the apertures found to occupy ·465 of the total area of the screen.

It was concluded therefore as the result of the experiments:

1. That the transmission as measured by the photometer box was equal to the ratio of the sum of the areas of the apertures of the screen to its total area, and therefore could be considered to be the true transmission of the screen, and

2. That the much smaller transmission of the screen, when used in front of the object-glass of a telescope to diminish the apparent brightness of a star, is satisfactorily accounted for by the loss of light caused by diffraction under these circumstances.

3. That screens used for this purpose should have their con-

stants determined by special experiments of the nature of those just detailed, and that their photometric use should then be limited to the reduction of the light of bodies possessing a small angular magnitude.

Allegheny Observatory, July 13, 1885

ART. XXVIII.—*Geological Relations of the Gypsum Deposits in Cayuga County, N. Y.;* by S. G. WILLIAMS.

IT is well known to geologists that the strata containing the gypsum deposits of New York, ranging from Madison County westward, at the time of the State Geological Survey were all assigned to the third of the four groups into which the Salina Period was divided; and that these gypsum deposits were described as isolated masses of possibly concretionary character contained in the enclosing beds. The gypsum beds a short distance north of Union Springs, Cayuga County, are much the most extensive of these deposits, and as they afford a fertilizer of approved quality, they have been largely laid open by workings prosecuted during more than fifty years. A rock cutting made by the Cayuga Lake Railroad, along the bank of the lake within eighty rods of the largest quarries, has also laid open the strata which underlie the gypsum beds. A recent examination of the gypsum beds and their enclosing strata, made with these more recently afforded aids to investigation, and accompanied by fortunate discoveries of somewhat abundant though often badly preserved fossils, has not only shown that the gypsum in this locality exists in continuous beds of a considerable degree of regularity, but has also afforded reasons for believing that this portion of the gypseous series belongs rather with the Lower Helderberg than with the Salina.

A section at this point eastward, and thus nearly in the line of strike, from the level of Cayuga Lake to the top of the Oriskany sandstone, is about 114 feet thick, and consists of the following members, numbered upward from the lake level:

No. 8. Oriskany sandstone in a single fossiliferous seam, 3 feet 8″.
" 7. Drab limestone, upper beds with thin undulating laminæ: exposed, 10 feet.
" 6. Limestone revealed only in occasional outcrops, leveled with Locke-level, about 46 "
" 5. Thick-bedded blue limestone, containing *Meristella lævis*, *Orthis oblata*, small form, *Rhynchonella semiplicata*, *Strophodonta varistriata*, etc., 10 "
" 4. Drab limestone, holding a branching fucoid and *Nucleospira ventricosa*, 4 "

No. 3. Measures concealed, leveled with Locke-level, 20 feet.
" 2. Thin-bedded blue limestone terminating below with a seam 2 feet thick, 4 "
" 1. Drab limestone unevenly bedded with two or three thin blue seams, to Lake-level, 16 "

The mean of four readings with the aneroid agrees well with the total thickness here given. The beds No. 1 have yielded fossils at three points, all at about the same level near the middle of the series, viz: one head of *Eurypterus remipes*, and tolerably abundant *Leperditia alta, Nucleospira ventricosa* and *Meristella bisulcata.* Although the two species last named are but imperfectly preserved, still the comparison of a considerable number of specimens, among which are some fair casts of the interior, leaves no good reason to doubt their specific identity; besides which, my own opinion is confirmed by that of the experienced paleontologist, Mr. R. P. Whitfield, to whom a number of the specimens were submitted.

Besides the irregularities in the bedding of No. 1 mentioned in the section, the strata in this region are occasionally affected by local disturbances. The most common of these is a sudden tilting of the beds at a low angle, continuing sometimes a number of rods, which is caused apparently by a failure of support from below. One such disturbance occurs near the middle of No. 1, causing the beds to dip very perceptibly to the southeast for a short distance. More considerable disturbances of like character affect the Corniferous limestones of Union Springs, about two miles south of the plaster quarries, two of which have come under my notice. One of these, which was mentioned and figured in the Report on the 3d District, and by which the limestone is caused to pitch suddenly south at an angle of 13°, has within the last two years, been more fully developed by the opening of a large quarry immediately south of the disturbed beds. In this quarry, which is capped by a considerable thickness of Marcellus shales with a band of concretionary limestone, is revealed a flat-topped anticlinal arch with an E.-W. strike, the southern limit of which has not yet been reached in quarrying, while the northward dipping side with an angle of 20° is near the junction with the southward dipping beds described by Vanuxem. In one of the most extensive plaster quarries also, there occurs a gentle anticlinal with meridional strike, through which the present working floor of this quarry and the one to the north of it, is made to dip eastward at a small angle as far as the workings extend; and if this dip continues, it will increase by a number of feet the space between Nos. 2 and 4 which was found by leveling to be 20 feet. The knowledge of these occasional irregularities demanded caution in assigning the gypseous series, with an average

thickness of about 25 feet, to its proper place in the section, especially since the space of nearly a quarter of a mile between the fossil-bearing beds on the lake shore and the gypsum quarries, is concealed by drift. Fortunately the valley of a brook, separated only by a single field from the nearest quarry at this point, affords a continuous line of outcrop from the fossiliferous limestones on the lake to the top of No. 2, by which its continuity is assured. The top of No. 2 is 20 feet above the lake-level, which is also the height of the floor of the nearest plaster quarry. Add to this the fact that the character of No. 2 corresponds with that of the bed of tough blue limestone which forms the bottom of all the southern quarries, and there is no reason to doubt that the plaster series belongs in the covered space between Nos. 2 and 4, widened probably by the local easterly dip mentioned above,—a position to which, in the absence of any local irregularities, it would be unhesitatingly assigned by any geologist.

There is then no doubt that the gypsum deposits here form a part of the fossiliferous series, lying above beds containing *Eurypterus*, *Leperditia alta*, *Nucleospira ventricosa* and *Meristella bisulcata*, and below, or in close connection with, beds containing well-marked fossils of the Pentamerus and Shaly Limestone horizons of the Lower Helderberg. It should also be borne in mind that, apart from the gypsum beds, the entire section from the lake level to the Oriskany sandstone, is made up of drab limestones with frequent blue seams, sometimes of considerable thickness, as in No. 5. Some of these limestones, both at the bottom and top of the series, are highly laminated, showing thin layers of slightly different colors, and nearly all hold a considerable amount of impurities. A test which was made of No. 1 showed 25½ per cent of insoluble matter, so fine as to cause difficulty in filtering; while some of the higher beds above No. 5 are said to have been burned for hydraulic lime at an early day, yielding a cement of good quality, thus showing that they are probably no purer than No. 1. There is indeed, even if we set aside the fossil evidence now gained as to geological age, no such lithological change in the limestones as to warrant the reference of the lower portion of them to the Salina Period, and the upper part to the Lower Helderberg. Nor is it likely that any such reference would ever have been made had it not been for the presence of the gypsum deposits. These deposits in the regions both east and west of Cayuga county appear, from the state reports, to occupy a pretty definite place in strata bearing intimate relations with the shales of the salt group; to occur in irregular masses enclosed in marly shales whose lamination they sometimes share and sometimes disturb; and to be divided often into two "ranges" by a pecu-

liar porous or vesicular lime rock, or by shaly limestones, holding indications in hopper-shaped accretions of the action of saline waters. It was natural, therefore, in the absence of any evidence to the contrary, that the occurrence of gypsum in any additional locality not obviously removed from its usual horizon, should lead to the reference of both gypsum and the accompanying strata to that usual horizon. A brief description of the gypseous series here however will show, I think, that its structure bears no very close resemblance to that of the gypsum of the Salina Period; that its character is intimately related with that of the accompanying limestones; and that both character and structure tend to indicate for it a different geological horizon, if not a different origin, from that of most of the other gypsum deposits.

It may be said at the outset that the gypsum deposits of this region are not irregular masses: they have no relations therefore with marly strata surrounding and enclosing them; they are not associated with any "vermicular lime rock" within them, nor with anything answering to the 4th or Magnesian division of the Salina lying above them. In one point only do they bear a superficial resemblance to the deposits elsewhere: they occur in two "ranges," or rather beds, separated from each other, however, not by shaly or vesicular lime rock, but by a bed called *slate* by the quarrymen, made up of thin seams of gypsum, interlaminated with other layers of shale.

The gypseous series here, as revealed by workings mostly confined to the vicinity of the lake, but extended northeastward nearly three miles by occasional pits, from Yawger's Quarry on the south, the bottom of which is 25 feet below the lake level, to the northernmost ones of Mr. Fitch and others which are more than 100 feet above the lake, has a very uniform character, consisting as it does of three persistent and tolerably regular members. *First* occurs the lower seam of gypsum, highly laminated and separating into several distinct layers, somewhat harder than the upper seam, and of an usual thickness of seven feet, varying but little in this respect. The existence of this seam appears, from the statement of the owner of most of the quarries, not to have been suspected forty years ago. *Second:* upon this rests a stratum usually about three feet thick called *slate* by the quarrymen, consisting of alternating laminæ of gypsum and shaly matter, and said to be gypseous enough in the northernmost quarries (which are now little worked) to be ground for plaster. *Third:* the upper gypsum bed which closes the series, varies much in thickness, from nothing to upwards of 20 feet, averaging possibly 15 feet. Its variability in thickness is probably due mostly to denudation, since it is capped by yellow drift clay in nearly

every place where it is laid open by workings; the only exception that I could find being in the two northern quarries called the Fitch quarries and which are but a few rods distant from each other. Here the gypsum, which occurs in several thick layers not well enough revealed to admit of definite measurement but showing at least 12 feet, has above it about six feet of black *dirt* resembling an impure leached gypsum which it probably is, eight feet of very shaly drab limestone, and three feet of somewhat firmer drab limestone containing *Spirifera Vanuxemi*, a Lingula apparently undescribed, somewhat smaller and more oval than *L. spathata*, fragmentary impressions of a Rhynchonella not sufficient for determination but having some resemblance to *R. mutabilis*, and a slightly tapering fragment $2\frac{4}{5}$ inches long, nearly an inch wide at the widest end, and $\frac{7}{8}$ inch wide at the narrowest, and marked lengthwise with 12 shallow furrows; this may possibly prove to be a plant allied to Calamites. These fossils have been discovered since the first part of this paper was written; but their bearing on the question of geological age will be obvious. The upper gypsum bed shows little disposition to separate into distinct layers save in the northern quarries, and is softer and somewhat less dense than the seven-foot bed. It was thought also to be of better quality until analysis showed it to be nowise superior. All the members of the series show occasionally small spots and thin scale-like laminæ of sulphur, more especially on dirt seams. The upper bed, I am told, contains more of this sulphur than the lower, and the slate seam more than either. The gypsum of both seams varies from a light to a somewhat dark gray.

The gypseous series here shows therefore no tendency to form isolated masses, save where denudation may give it that appearance, in which case it is enveloped in drift clay. Two proprietors of long experience however inform me that the entire set of beds is occasionally cut across by what are called "mud seams" from one to five feet wide, that the mud seams are often of thin-laminated structure and sometimes contain a little gypsum and selenite, and that the gypsum beds abut against them regularly on both sides. The only example of this kind of replacement that has come under my notice, was in the edge of one of the quarries where, at the time of my last visit, the lower gypsum bed and the "slate," the only members there present, had suddenly given place below to black thinly-laminated mud, and above to harder thin-bedded ferruginous shale, the "mud seams" abutting against the gypsum and "slate" in a reëntrant fashion. The lamination of the mud appeared to correspond to that of the gypsum against which it abutted, and one block was hard gypsum at one end

and black laminated mud at the other, although elsewhere the gypsum and mud were separated by irregular joint-like cracks. I was inclined to attribute the change to the action of water penetrating to the beds through crevices in the clay cover.

The limestone of that part of the series in question which encloses the gypsum beds is of a prevailing drab or ash color, with a few blue seams, of which No. 2 of the section is the only important one. It is often highly laminated; has a considerable amount of impurities as has already been said; and by reason of the earthy character of these impurities, it shows such a disposition to absorb water as to unfit it for all but the roughest purposes. A fragment of No. 1 gained in weight 3 per cent by two hours soaking, while a like fragment of the blue limestone No. 2 showed no appreciable gain in the same time.

The character of the limestones just described seems to me to throw light on the question of the origin of the gypsum beds; these I think have obviously been formed from the earthy drab limestones of the horizon at which they occur, as the result of the action upon them of acid waters originating in sulphur springs, which are still somewhat abundant in this region, and which it may be presumed were more abundant at an earlier geological date. The porous character of the drab-colored limestones would facilitate such a transformation, under favorable circumstances; while the imperviousness of the blue limestone which underlies the series would limit it below. Reasons for this opinion as to origin may be found: first, in the striking similarity in structure between the lower gypsum and the associated drab limestones, both having the same highly laminated character, while both the lower gypsum and the northern part of the upper, are also distinctly bedded; second, in the structure of the intermediate bed, containing as it does alternate layers of gypsum and shale, as if whatever was lime in an impure shaly limestone had been transformed to gypsum, leaving the remainder unchanged; third, in the presence in all the gypsum beds of native sulphur which would be difficult to account for on any theory of origin which should not include the action of sulphuretted waters; fourth, in the composition of the gypsum itself, which is gray, and like the limestones somewhat impure, containing in commercial samples an average of 80½ per cent of lime sulphate, with 14 per cent of earthy matter, 5 per cent of lime and magnesian carbonates, and, quite significantly, .6 per cent of lime phosphate and organic matter, these last ingredients suggesting an organic origin, while the residual lime and magnesia point to the probable original condition of the deposit. It may be added in this connection that

in the limestone No. 1, in close proximity to one of the fossil localities and nearly at the same level, occurs a small isolated mass of decomposed gypsum possibly 10 cubic feet in dimensions, which is due apparently to the agency of a small sulphurous percolation now extinct.

I believe, therefore, that the structure of the great gypsum deposits of Cayuga Co. separates them sharply from those existing elsewhere in New York in strata of the Salina Period; and that their association with limestones, both below and above them, containing fossils of the lower divisions of the Lower Helderberg, as well as the nearly uniform character of the rock series from the lake level to the Oriskany Sandstone, vindicates for them a place in the lower portion of the Lower Helderberg in which I include the Water Lime Group.

A word in conclusion as to the dip of the strata at this point. It will be seen that the top of the Lower Helderberg limestones in the line of section is 110 feet above the lake. About 2½ miles south of this point, these limestones disappear beneath the lake, giving a dip of more than 40 feet per mile. This corresponds with some determinations of dip published in this Journal, October, 1883, made on a belt fifty miles long from east to west, and from six to ten miles wide, lying six miles south of Union Springs. I have at present no data for ascertaining whether the effective dip in the intermediate space corresponds with these determinations.

ART. XXIX.—*On the variation of the Magnetic Permeability of Nickel at different temperatures;* by CHARLES A. PERKINS.

IN the year 1820 Oersted published his discovery of the action of an electric current upon a magnet and Arago discovered that the current would magnetize an iron bar. The same year Ampère proposed his theory of molecular currents to explain both phenomena. Poisson soon after elaborated a theory based on the existence of magnetic fluids, but the tendency has always been toward the acceptance of Ampère's hypothesis and later physicists have striven principally to modify and enlarge it, to make it correspond with the known facts of magnetism.

It was upon this as a basis, that Weber constructed his theory assuming the existence of molecular currents which an inductive force could so arrange as to produce a magnet, while they resisted this tendency to a certain degree.

With Maxwell's additions this theory gives a rational formula which agrees much better with the known facts than do many of the empirical formulas that have been proposed.

All the formulas introduce certain constants depending on the relation between the magnetizing force and the magnetism produced, to find the value of which, as well as to test the theories themselves, experiments have been made repeatedly.

The first series of experiments undertaken in a really scientific manner are those made by Lentz and Jacobi.*

The method employed by them was to place a bar of iron in a helix of wire and to pass through this a current whose strength was measured by the attraction between two spirals through which it passed. The magnetization was measured by the induced current produced in a coil surrounding the iron bar and the magnetizing spiral. In this way they arrived at the result that the total magnetization was proportional to the strength of the current.

Joule† discovered, in experimenting on the strength of electromagnets in 1839 and 1840, that he could not indefinitely increase their carrying power by an increase in the current, but that it approached a maximum as the iron became "saturated." He also clearly stated the law that the maximum is independent of the length of the magnet and varies directly with the smallest cross-section.

But although Joule's experiments established the fact that the magnetization of iron was not proportional to the current strength, yet the principle was already stated in a paper by Ritchie‡ in 1833, six years before the publication of Lentz and Jacobi's results.

He made use of two horse-shoe electromagnets of very different lengths and an armature, all of the same metal. Then putting a different number of coils of wire about each magnet, he connected both at the same time with a battery and found the shorter one had twice the power of the longer. Then keeping the connections the same he joined a larger battery and found that the long magnet had increased in proportion much more than the short one. He states that this is because the particles in the short magnet were arranged by the first current in the position to give the best effect, so that the stronger one could not add much to it.

Plücker§ found in 1848 that bodies which at a distance were attracted by a magnet were repelled when brought very close. This was true of charcoal and a number of organic substances. This was ascribed by him to the different rate of increase in magnetization as the magnetizing force increased, and proved not only that the magnetization was not proportional to the force but that it might be entirely reversed.

* Pogg. Ann., vol. xlvii, 1839.
† Phil. Mag., [4], vol. ii, 1851. Annals of Elec., vol. iv, 1839.
‡ Phil. Trans., vol. cxxiii. § Pogg. Ann., 1848.

Three years later, Faraday* in attempting to classify magnetic substances: glass, water, etc., found that the order was different at different distances from the magnet, showing that the increase of magnetization was not directly as the magnetizing force and that the law was different for different materials.

In 1849 Müller† undertook an extensive series of experiments fully establishing the fact that the magnetization of iron was a complicated function of the magnetizing force and proposed the equation $I=220d^{\frac{3}{4}}$ tan $\frac{m}{00005}d^n$ as sufficiently representing the experiments. His method consisted in observing the deflection of a needle by a bar of iron magnetized by a helix through which a measured current was passed.

In 1852 W. Weber‡ stated his theory of induced magnetism and proposed an equation based upon the same. He also made experiments to find the connection between the theory and observation. The method employed by him was the deflection of a needle by a long thin rod closely surrounded by a spiral which extended also beyond the rod. The magnetizing current was measured and its force calculated so that the whole might be reduced to absolute measure. He found a fair agreement with the theory and determined the maximum magnetization possible for iron.

Beetz§ in 1860 showed most conclusively that iron had a maximum magnetization and he perhaps approached it as nearly as is possible. A fine line was made on a varnished silver wire and iron deposited electrolytically while under the influence of a magnetizing force and thus the molecules were so arranged as to give the greatest effect. In this way a high degree of permanent magnetization was given to the filament of pure iron and a stronger magnetizing force only served to slightly increase the temporary magnetization.

More recent experiments‖ have showed that the law of variation proportional to the current was not true even for weak currents, the increase being at first more rapid, then less so, than this law would allow. This was most noticeable in the experiments of Quintus Icilius¶ to which attention was forcibly called by Stoletow in a paper which appeared in Pogg. Ann., vol. cxlvi. Instead of a cylindrical bar he used a very long ellipsoid, thus avoiding the error of distribution neglected by his predecessors. The magnetism was measured by the direct action on a needle or by the induced current produced in a helix about the inducing spiral.

Stoletow's paper contains valuable discussions of previous experiments and important results obtained by himself. The

* Exp. Res., vol. iii, p. 503, 1851.
‡ Pogg. Ann., vol. lxxxvii.
‖ Wiedemann Galvanismus, vol. ii, p. 350.
† Pogg. Ann., vol. lxxix.
§ Pogg. Ann., vol. ciii.
¶ Pogg. Ann., vol. cxxi.

method used by him was suggested and developed by Kirchhoff* in 1870, and consisted in magnetizing a ring of metal by a coil about its entire length and then measuring the current produced in a secondary coil when the primary current was reversed. This method possesses decided advantages over all previously used in important particulars and is capable of giving results of great regularity. He arrived at results similar to those of Icilius, but found certain irregularities for which he was not able to account.

The same year Prof. Rowland† had made experiments using the same general method, but introducing various modifications. An earth inductor was placed in the secondary circuit by which the induced currents could be directly compared with those produced by a coil rotated under the influence of the earth's action and thus reduced immediately to absolute measure. In the same circuit was included a small coil of wire, sliding upon a magnet. By the motion of this coil the galvanometer needle is under perfect control and can be instantly stopped at the middle of the scale. He wound the primary coil directly upon the ring, while in Stoletow's experiments wooden rings were placed inside the coil.

Rowland also suggested plotting the results with respect to the magnetization and the magnetic permeability as coördinates, thus obtaining a curve which gave a definite value for the maximum magnetization possible.

The results reached by him in the experiments on iron were as follows:

For weak forces all the magnetism is temporary.

The value of the permeability rises very rapidly at first, reaching a maximum when the magnetization has the value $\mathfrak{B}=6000$ and then diminishes, approaching 0 as $\mathfrak{B}=17500$. This, therefore, is about the maximum magnetization of ordinary iron, giving a sustaining weight of 354 lbs. per square inch of least cross section.

(This is in C. G. S. units. The original results were on the M. G. S. system, and as the dimensions of $\mathfrak{B}$ are $[L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}]$, the original figures are to be divided by ten. μ is of zero dimensions.)

The curve in form resembles a slightly inclined parabola, but is more accurately represented by the equation

$$\mu=2475 \sin\left(\frac{\mathfrak{B}+1{\cdot}094\mu+500}{100}\right).$$

In some experiments the curve finally became curvex toward the axis, which was ascribed to unhomogeneity in the ring but

* Pogg. Ergz. Bd., vol. v.

† Phil. Mag., August, 1873.

may have been due to a departure from the above law which takes place on approaching the limit.

The same thing has since been observed by Fromme, who made use of the same method and ascribed the result to the latter cause.

In these experiments Rowland also investigated the magnetism of nickel and a law for the variation of μ was discovered similar to that holding in the case of iron. More recently* he has made further experiments on nickel and has added cobalt to the list of substances following the same general law. On account of the difficulty of procuring these metals in a pure condition, fewer experiments have been made with them than with iron, but the results, as far as found, agreed with those for iron.

In 1853 Plücker† experimented on a large number of bodies, and although the experiments were complicated by using an electro-magnet as the magnetizing force, he attempted, as far as possible, to eliminate this effect by using the law of increase in the case of iron. Only a few points were determined to fix the curve of magnetization, but the results found showed that all the substances experimented on approached a definite maximum which for iron, nickel and cobalt was relatively: Fe 1000, Co 918, Ni 322, values which are in very good agreement with those since found.

Arndtsen‡ also, using Weber's diamagnetometer, found that nickel soon approached a maximum magnetization and reducing the values to absolute measure stated that for very weak magnetizing forces the magnetism of nickel was five times that given by Weber's formula for iron.

All the experiments above mentioned were made at ordinary temperatures, but Faraday§ found that by heating in olive oil, the electro-magnetic properties of iron were hardly changed. Nickel suffered a considerable decrease, while cobalt increased in heating.

The later experiments by Rowland, referred to above, were made especially with reference to this point and show a very interesting property of nickel, viz: that for weak magnetizing forces its permeability is much increased by heating from 15° to 220° Centigrade, while for stronger forces it is decreased. The effect on iron is slight and cobalt has its permeability increased for all magnetizing forces used, showing the accuracy of Faraday's conclusions.

More recent experiments by Fromme, Riecke, Ettinghausen and others have confirmed the accuracy of these conclusions and at the same time have developed new facts in relation to the subject.

* Phil. Mag., November, 1874.
† Pogg. Ann., vol. xci.
‡ Pogg. Ann., vol. civ.
§ Exp. Res., xxx, 3424, 1855.

Fromme* has shown that by repeatedly magnetizing in the same direction, the permanent magnetism of an iron bar is increased, while the total magnetism is not much altered, according with Maxwell's idea of a force which causes a molecule to return to the position from which it has been displaced by the magnetizing force, unless the force exceeds a certain amount.

In 1880 Wassmuth,† in a research on the effect of heat on the magnetization coefficient of iron, found that for weak forces it was increased by heating from 20° to 146°, but it reached a maximum sooner and the maximum magnetization was less by about three per cent.

This result corresponds to that found by Rowland and shows that the difference between this metal and nickel is only one of degree.

Bauer‡ also, using both a ring and a bar of iron and measuring the induced current produced by reversing the magnetism, found that the same law continued at much higher temperatures. For weak magnetizing forces the coefficient rises rapidly, reaching a maximum as the metal becomes red hot and then rapidly falls.

Following the suggestions made in Rowland's paper, the present series of experiments upon nickel was undertaken with a view to secure complete curves at a number of temperatures, showing the relation between the permeability and the magnetization, and thus if possible, determine the law by which it changed on passing from one temperature to another.

The rings used were from commercial rolled nickel§ and therefore not as pure as those used by Rowland. Still they were quite pure, as a chemical examination showed.

Their physical and chemical properties were as follows:

Ring.	Spec. grav.	Weight.	Cross sec.	Mean diam.	
I.	8·731	34·855	·196	6·440	Contain some C and Si.
II.	8·746	25·514	·198	4·696	A small precipitate was produced by H_2S.
III.	8·394	7·814	·0624	4·427	Contain no Co or Fe.

Rings I and II were from the same piece. Before using they were carefully annealed by heating to redness in a crucible filled with sand, which was allowed to cool as the fire died out.

As the temperatures employed were so high as to destroy ordinary insulation the rings were covered with a thin layer of asbestos paper, then a coil of wire was wound, then another

* Pogg. Ergzbd., vol. vii.
† Sitzb. der k. Akad., vol. lxxxii.
‡ Ann. der Phys. u. Chem., vol. xi.
§ Made by Wharton, Philadelphia.

layer of asbestos, etc., by which means an insulation was produced which was not affected by heating to a red heat.

The primary coil was in two layers, and as the second layer was not wound immediately upon the ring, a certain error was introduced, the magnetism being apparently increased by an amount which was in one case $\frac{1}{40}$, and varied from this to $\frac{1}{600}$, but in rings I and II it in no case exceeded $\frac{1}{180}$. Whenever it did so in ring III a correction was made.

The magnetizing current was measured by a tangent galvanometer constructed by Rowland. Its constants were computed from its dimensions. The circle was 21 cm. in diameter and graduated to quarter degrees. Six coils were used, each containing about three times as many turns as the next preceding. From six to twenty-four cells of Bunsen chromic acid battery furnished the current, which was further modified by the introduction of a variable resistance.

The galvanometer for measuring the induced currents had a sensitively adjusted astatic needle whose directive force was given by a magnet placed below. It had a period of about nine seconds, and was deflected by one turn of the earth inductor through nearly four degrees. It was read by a telescope and scale one meter distant. The earth inductor was introduced for the purpose of comparing the induced current with one of known strength and the winding included a total area of 20,716 sq. c. m. It was adjusted in a plane perpendicular to the magnetic meridian and could be turned through an arc of 180°. As in Rowland's experiments, a coil of wire sliding upon a magnet was introduced into the circuit, to control the vibrations of the needle.

The ring was placed in a bath of kerosene for low temperatures, for 100° in one of paraffine and heated in a steam bath. At higher temperatures it was placed in an oil bath which was designed for the comparison of thermometers at high temperatures. This was made of very heavy copper castings and held several liters so that its temperature, when kept up by a Bunsen burner, would remain constant for a long time, rarely varying more than four or five degrees during the experiment which usually took several hours. The temperature was determined by a mercury thermometer.

The course of each experiment was as follows:

The current was allowed to run for some minutes through a shunt circuit of about the same resistance as the ring, then turned through the ring and the readings of the tangent galvanometer made.

Then the deflections of the mirror galvanometer by the induced currents were measured, four readings being taken for the temporary magnetism and four for the total. Then another measurement of the primary current was made.

In the case of very strong currents which would heat the ring rapidly the measurements of temporary magnetism were omitted and the tangent galvanometer reading was taken between the measurements of the induced current. In measuring the temporary magnetism, the current, instead of being broken, was turned into the shunt, so that the battery strength should remain constant.

Readings of the thermometer were made before and after each observation and the deflection produced by the earth inductor was taken each time, though in most of the experiments this was so uniform that the mean of all the values was taken as the true value.

Since the rings used in this experiment were of rectangular section, the solution given by Kirchhoff does not accurately hold, but the theory is equally simple.

Let $\mathfrak{B}$ = magnetic induction through unit area.
μ = magnetic permeability.
H = horizontal component of earth's magnetism at the earth inductor.
H′ = horizontal component of earth's magnetism at tangent galvanometer.
i = intensity of the primary current.
A = total area included by the earth inductor.
A′ = area of the section of the ring.
n = whole number of windings about the ring in the primary circuit.
n' = whole number of windings in the secondary circuit.
Q = current due to reversing earth inductor.
Q′ = current due to reversing magnetism of the ring.
$\mathfrak{S}$ = deflection of galvanometer produced by Q′.
$\mathfrak{S}'$ = deflection of galvanometer produced by Q.
R = mean radius of the ring.

Then the magnetic induction at any point in the ring is $\frac{2ni\mu}{\rho}$ and the total induction through any section is:

$$2ni\mu b\int_{-\frac{a}{2}}^{\frac{a}{2}}\frac{1}{\mathrm{R}-x}dx$$

where ρ is the distance of any point from the axis of the ring and a and b are the sides of the rectangular section.

The value of the integral is

$$\log\frac{\mathrm{R}+\frac{a}{2}}{\mathrm{R}-\frac{a}{2}}=\frac{a}{\mathrm{R}}\left(1+\tfrac{1}{3}\frac{a^2}{(2\mathrm{R})^2}+\tfrac{1}{5}\frac{a^4}{(2\mathrm{R})^4}+\text{etc.}\right)$$

$$\therefore\ \mathfrak{B}=\frac{2ni\mu}{\mathrm{R}}\left(1+\tfrac{1}{3}\frac{a^2}{(2\mathrm{R})^2}+\text{etc.}\right)$$

The value of the second term of the series never reaches $\frac{1}{300}$ in the rings used and hence is entirely imperceptible.

$$Q \propto 2AH \qquad = C \sin \vartheta'$$

$$Q' \propto 2n'A'\mathfrak{B} \propto \frac{4nn'\mu i A'}{R} = C \sin \vartheta$$

$$i = \frac{H'}{G} \tan \delta$$

$$\frac{\sin \vartheta}{\sin \vartheta'} = \frac{2nn'\mu}{R} \frac{A'}{A} \frac{H'}{H} \frac{\tan \delta}{G}$$

$$\therefore \mu = \tfrac{1}{2} \frac{R}{nn'} \frac{A}{A'} \frac{H}{H'} \frac{G}{\tan \delta} \frac{\sin \vartheta}{\sin \vartheta'}$$

$$\text{and } \mathfrak{B} = \frac{HA}{n'A'} \frac{\sin \vartheta}{\sin \vartheta'}$$

All measurements given in this paper are in C. G. S. units.

The values of some of the quantities used in the experiment are as follows:

$$H = \cdot 1873$$
$$H' = \cdot 1883$$

The values of n are

Ring	I	205
Ring	II	183
Ring	III	292
		169

To vary n', several sets of coils were wound about each ring containing from eight to ninety-five turns, according to the currents used and the cross section of the ring. The different coils in the same ring were of the same resistance, but any slight changes in the adjustment would be eliminated by the earth inductor.

In one case the ring was cooled to $-17°$ by means of ice and calcium chloride, but the change produced by this was so small that it was not considered worth doing in the other cases.

The results obtained for the three rings were as follows: (μ_1 is the permeability for temporary magnetism and μ_{1+2} for total.)

TABLE I.—Ring No. I.

Temperature=22°.			Temperature=100°.			Temperature=205°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
80·	83·9	29·2	105·	107·	58·	127·	141·	87·2
74·	74·	33·4	118·	160·	563·	148·	202·	559·
83·	109·	278·	125·	178·	724·	149·	240·	977·
92·	149·	593·	120·	217·	1720·	138·	250·	1790·
98·	199·	1460·	115·	204·	2400·	130·	228·	2280·
102·	200·	2180·	107·	187·	2890·	117·	188·	2960·
100·	194·	2510·	94·	151·	3710·	100·	150·	3580·
90·	155·	3700·	----	100·	4940·	----	131·	3880·
----	102·	5260·	----	----	----	----	97·4	4370·

Temperature=262°.			Temperature=314°.			Temperature high.	
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_{1+2}	$\mathfrak{B}$
147·	156·	90·5	185.	205·	93·9	169·	3120·
168·	243·	583·	210·	353·	677·	132·	3580·
173·	278·	906·	197·	359·	1050·	170·	2920·
160·	291·	1400·	173·	319·	1360·	123·	3300·
144·	246·	2170·	137·	209·	2080·	133·	2900·
121·	189·	2850·	109·	154·	2360·	103·	2820·
97·	140·	3300·	78·	104·	2460·	117·	2120·
----	111·	3630·	----	85·	2490·	63·1	1780·
----	84·1	3690·	----	----	----	23·6	428·
----	----	----	----	----	----	5.9	107·

See plate (p. 229) figure 1 where the continuous lines correspond to the total magnetism, and the broken lines to the temporary. The points for the different curves of temporary magnetism, have distinctive marks.

TABLE II.—Ring No. II.

Temperature=21°.			Temperature=100°.			Temperature=197°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
64·	63·9	49·6	87.	90·5	59·3	111·	114·	70·7
65·	72·6	137·	92·	125·	402·	118·	165·	485·
71·	93·	303·	98·	157·	806·	120·	208·	850·
79·	136·	710·	99·	184·	1570·	119·	209·	1730·
67·	168·	1460·	98·	176·	2150·	113·	194·	2210·
86·	164·	2050·	93·	158·	2790·	102·	163·	2810·
83·	147·	2710·	81·	126·	3820·	84·	119·	3660·
78·	125·	3030·	----	84·1	4910·	81·	119·	3710·
----	83·	5090·	----	----	----	----	80·1	4420·

Temperature=269°.			Temperature=298°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
126·	128·	41·3	140.	161·	60·3
129·	150·	163·	161·	184·	186·
138·	215·	560·	157·	273·	759·
145·	235·	844·	155·	275·	1050·
141·	248·	1110·	151·	268·	1230·
127·	222·	1860·	124·	218·	1820·
114·	184·	2390·	110·	169·	2330·
80·	110·	3350·	77·	104·	2930·
----	83·9	3620·	----	69·2	2810·

TABLE III.—Ring No. III.

Temperature=−17°.			Temperature=15°.			Temperature=100°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
58·	63·3	92·4	69·	83·2	117·	127·	143·	181·
69·	106·	378·	80·	135·	438·	139·	212·	677·
78·	165·	970·	95·	193·	1120·	152·	248·	1390·
87·	201·	2070·	105·	217·	2290·	139·	259·	2280·
91·	192·	2820·	104·	200·	3040·	130·	219·	3270·
87·	166·	3580·	103·	206·	2860·	113·	186·	3750·
73·	129·	4320·	100·	178·	3580·	90·	139·	4380·
60·	100·	4670·	80·	130·	4320·	70·	105·	4750·
37·	58·2	5280·	67·	106·	4640·	42·	59·4	5160·
----	----	----	42·	62·1	5220·	----	----	----

Temperature=210°.			Temperature=300°.		
μ_1	μ_{1+2}	$\mathfrak{B}$	μ_1	μ_{1+2}	$\mathfrak{B}$
191·	191·	139·	268·	271·	193·
187·	227·	339·	245·	349·	557·
197·	266·	727·	245·	388·	1040·
190·	295·	1890·	218·	337·	1820·
175·	286·	2300·	169·	274·	2140·
161·	246·	2800·	158·	226·	2380·
111·	157·	3740·	83·	113·	2780·
100·	139·	3890·	----	----	----
54·	68·3	3800·	----	----	----

Ring III was re-wound between the third and fourth experiments, and owing no doubt to a contact in the primary coil, the later values of μ were too small. In the fourth and fifth series, as given in the table, μ is multiplied by $1\frac{1}{6}$. On this account these experiments are of less value, but the error does not affect the determination of the maximum value of $\mathfrak{B}$ as given later.

The last series in table I was taken at varying temperatures above the previous. To secure this the ring, properly insulated, was placed on a plate of copper in a brass vessel and sand piled upon it. The whole was then heated slowly, and at intervals very rapid observations were made of the magnetic condition of the ring, and this was continued till the metal was nearly non-magnetic. The points thus found are indicated on the diagram. At the end the bottom of the vessel seemed slightly red.

An examination of the curves shows—

I. A rise nearly in a straight line at first and more rapid as the temperature is higher.

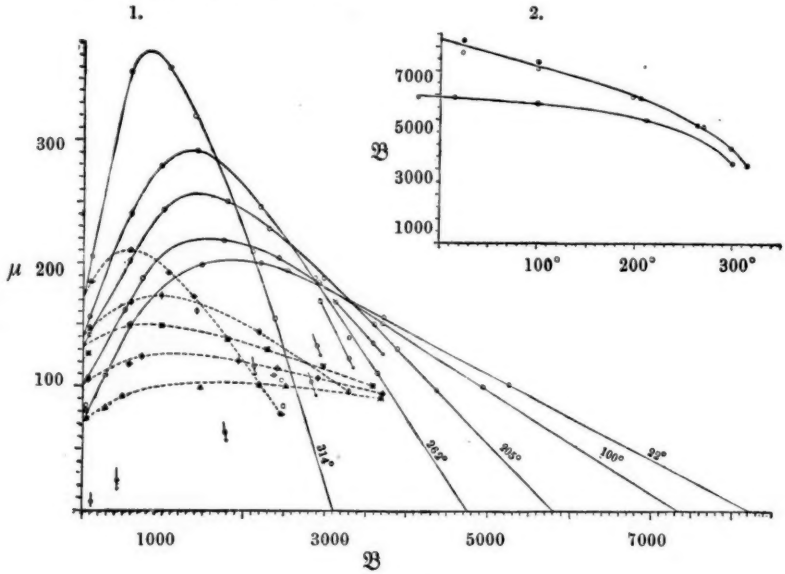
II. A sharp curvature, the sharper as the temperature is higher.

III. A descent which also is more abrupt as the temperature rises.

There was a limit to the extent to which the current could

be increased on account of the heating of the ring. In one or two cases where a strong current was used this effect was so marked that a smaller value of $\mathfrak{B}$ was found for the stronger current than for the weaker, which could not be due simply to the increase of the magnetic field.

The latter part of the curves is a nearly straight line, and when this is prolonged to cut the axis it gives the value which $\mathfrak{B}$ approaches as a maximum for all magnetizing forces here available. It is to be expected that the form of the curve will change as it approaches the axis, to which it no doubt becomes finally parallel, since $\mu=1$ for non-magnetic bodies, and in no known case is as low as zero.



Such an inflection was observed in the curve for iron by Rowland and by Fromme, as before stated.

Faraday* also found that at very high temperatures nickel still possessed the power of being attracted by his electro-magnet, so that it is doubtful whether the curve crosses the line $\mu=1$.

But although $\mathfrak{B}$ may never reach a maximum, yet the quantity found by prolonging the curve to the axis gives the law of the change of $\mathfrak{B}$ with varying temperatures and is the highest value of $\mathfrak{B}$ that can be reached by ordinary magnetizing forces,

* Exp. Res., vol. iii, p. 55.

the metal becoming practically non-magnetic in this neighborhood.

The values thus found for these rings are given here:

I	$\mathfrak{B}$,	8230	7250	6020	4810	3300
	temp.,	22°	100°	205°	262°	314°
II	$\mathfrak{B}$,	7820	7180	5950	4740	3820
	temp.,	21°	100°	197°	269°	298°
III	$\mathfrak{B}$,	5860	5860	5660	5000	3200
	temp.,	−17°	15°	100°	210°	300°

It will be seen that the values for rings I and II are as nearly equal as can be determined by this method, and they are therefore shown on the same curve where the values found for ring I are indicated by the crossed circles. Those for ring III vary somewhat less rapidly and lie constantly below the others.

This difference seems to belong to the metal itself, as it will be seen on examination of the curves for the rings that μ of ring III goes down more rapidly than the others, though it rises to the same height as ring I and higher than ring II at the maximum point.

If this curve be continued it must not be allowed to meet the axis at a large angle, since this would indicate that the ring resisted infinitely the passage of the lines of force, but it must nearly coincide at the last part with the curve representing $\mathfrak{H}$, the magnetizing force.

This has been shown to be true by some observations made by A. Becquerel. H. Becquerel* states that in some unpublished experiments his father found that nickel is still slightly magnetic when heated to about 600°, though only about $\frac{1}{4000}$ as much so as at ordinary temperatures. Faraday's observations indicate the same thing.

The additional series made with ring I shows that the change is continuous beyond the points indicated in the diagram.

The tables given for μ and $\mathfrak{B}$ include also the permeability for temporary magnetism, the curves of which are drawn on the same diagram† with the total permeability. (Fig. 1.)

The permanent permeability may be readily found by subtracting one of these from the other.

These results are not nearly as regular as the others because the temporary magnetism varies with many circumstances, depending on the amount and direction of previous magnetization and on the number of times the magnetism has been reversed.

If the ring has previously been strongly magnetized the first few reversals cause a greater change of magnetism than the subsequent, and the same is true for temporary magnetism.

* Ann. de Chimie et de Phys. † In broken lines.

Also, if the ring has been for some time magnetized in one direction the temporary magnetism in this direction is less than in the opposite by an amount reaching twenty per cent in some cases, thus presenting an analogy to the so-called "fatigue" of metals under strain.*

The curves, however, serve to show that at the first all the magnetism is temporary. The permeability rises to a maximum at about the same place as that of total magnetization, then falls less rapidly and approaches the total, though it is evident that it can never reach it, since the permanent magnetism cannot be less for a high magnetizing force than for a lower at any given temperature.

It is to be noticed that the magnets used have no free poles. Hence the transient nature of the magnetism is due to the structural properties of the material and not to any demagnetizing influence of the poles.

Art. XXX.—*Enlargements of Hornblende Fragments;* by C. R. Van Hise.

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The nature of the process or processes by which fragmental rocks have become indurated is a question of great interest. The enlargement of grains of quartz, as first described by Sorby,† in 1880, has since been shown‡ to explain in large measure the consolidation of quartzose sandstones to quartzites. This enlargement consists in the deposition of silica about rolled grains of quartz, the new silica being optically continuous with the old grains, or in other words forming with each of the original fragments a single quartz individual. The secondary growth of rolled grains of feldspar in feldspathic sandstones in a corresponding manner, i. e., the addition of feldspar to rolled grains of feldspar, the two being parts of the same individuals, was also announced by me some time since,§ since which time the study of many sections of feldspathic sandstones from various localities has made it certain that fragmental particles of feldspar frequently enlarge by renewed growth just as do grains of quartz. It would seem that the enlargement of feldspar fragments plays an important part in the consolidation of many feldspathic fragmental rocks.

* Righi has observed the same effect (Beibl. vol. v), and it is also similar to the observation of Fromme in the case of repeated magnetization (p. 223).

† Proc. Geol. Soc. London, 1880, p. 62.

‡ R. D. Irving, Am. Jour. Sci., June, 1883. R. D. Irving and C. R. Van Hise in Bull. U. S. Geol. Sur., No. 8, 1884.

§ Am. Jour. Sci., III, xxvii, May, 1884.

Hornblende is one of the most important of the remaining rock-forming minerals. The object of the present paper is to describe briefly some unmistakeable enlargements of hornblende fragments that I have recently met with in studying certain peculiar conglomerates under the direction of Professor R. D. Irving.

At Ogishke Muncie and Cacaquabic lakes, in the northeastern part of Minnesota (T. 66 N., R. 6 W., and T. 66 N., R. 7 W.), occurs a great belt of conglomerate forming part of the Archæan schists of that region. The following extracts from field notes by Mr. W. M. Chauvenet, of the U. S. Geological Survey, are descriptive of this conglomerate: On Ogishke Muncie "the great conglomerate formation or bed shows no apparent dip or strike, but massive walls of green color with joints and great cracks. . . . The pebbles protrude in masses, the great pink, granite pebbles being most numerous, often as large as three feet through. Bright red jasper pebbles give the rock a handsome appearance. Quartzite and slate pebbles are also present." At another place on Ogishke Muncie Lake: "The conglomerate and greenish conglomeratic rock rise in contact forming a cliff 30 to 40 feet high, broken or irregular. There is no marked division between the two, the one blending with the other. No red jasper pebbles are here seen, which fact distinguishes this from the coarser conglomerate. The pebbles are black and green in a dense matrix of crystalline greenish rock." At Cacaquabic Lake: "The conglomerate appears on the shore. The conglomerate differs little in appearance from that at Ogishke Muncie. The pebbles are of many kinds but not so thickly scattered. Jasper pebbles are very common, granite, clear quartz, and slate pebbles are present. This was especially apparent upon weathered surfaces." At times the exposures become "a green, sub-crystalline, exceedingly tough rock, but appearing like a bedded sandstone in place. It is finely but distinctly banded, cleaving most readily along the bands. . . . Above this rock is a distinctly crystalline, greenish rock, weathering to green and red, having a conglomeratic appearance in places where red granitic grains are thickly scattered."

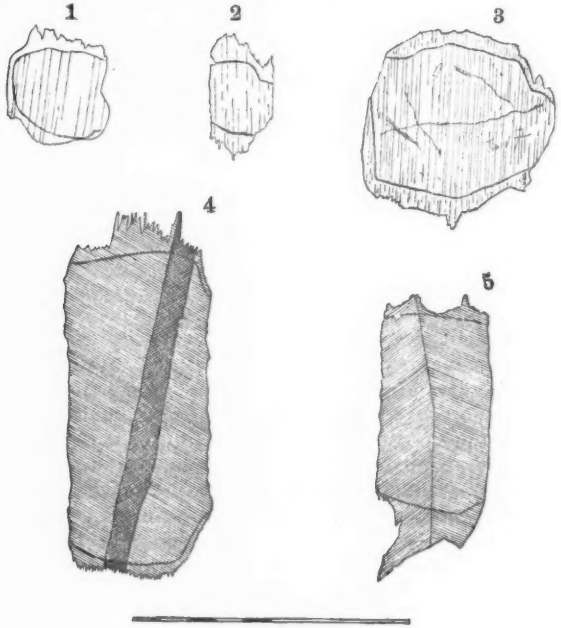
The best illustrations of the enlargements of hornblende yet found are in the denser parts of the conglomerate where the pebbles are comparatively rare. But these enlargements are present in nearly all sections cut from the matrix of this conglomerate throughout its entire known extent of a number of miles. As is to be expected, thin sections from different localities are quite different in appearance and composition. In many cases the finer parts of the sections are so exceedingly fine as to make it difficult to determine accurately the various

mineral constituents. However in these cases there are always present coarser grains of hornblende and feldspar, and the former commonly show enlargements.

The following is a brief description of one of the thin sections from the Ogishke Muncie rock, the mineral constituents of which are in the main quite clearly defined. The section is composed of a rather sparse fine ground-mass, through which are abundantly scattered medium-sized particles of hornblende and feldspar. Although cut from a conglomerate, at a first glance the section somewhat resembles that of a basic porphyry, but upon a closer examination its fragmental character is plain. With a high power, the ground-mass, which makes perhaps one-fourth of the section, appears to be composed of quartz and feldspar, with some kaolin and cryptocrystalline silica. The other four-fifths of the section are composed almost wholly of medium-sized grains of feldspar and hornblende in about equal proportions. The feldspar is in part orthoclase and in part plagioclase, the grains being usually more or less rounded and varying in size from those several millimeters across to those so small as to be lost in the fine matrix. Some of these grains of feldspar have apparently received enlargements. The hornblende is green or greenish yellow in color. In most cases it shows one cleavage and frequently is so cut as to develop nicely its two cleavages at the characteristic angle. In unpolarized light these individuals appear to be bounded by well defined lines, their apparent forms being ordinarily oval, rounded or rectangular. Much more rarely they possess crystalline outlines. In the polarized light, however, each grain of hornblende is found to extend beyond its apparent outline, as seen in the ordinary light, and to terminate commonly in a ragged outline with long projections. These present outlines are believed to be due to enlargements of the hornblende, the inner rounded, rounded rectangular, or crystalline outlines, bounding fragments of hornblende which after being deposited in the positions now occupied have taken a new growth. The lines of division between the fragmental cores and the exterior parts are distinct. They are due to numerous gas cavities and inclusions, the latter being mostly minute particles of ferrite, i. e. these lines of division are of the same nature as the corresponding lines in the enlargements of quartz fragments. When viewed in unpolarized light, in many cases there is little difference in the appearance of the cores and exterior portions of hornblende while frequently there is a marked difference. The added portions are paler in color, at times so much so as to give the impression that this part is quite different from the cores; yet,

between crossed nicols the lighter colored, most irregularly outlined exterior parts are optically continuous with the interiors of the grains. Quite frequently the individuals of hornblende are twinned, and in this case the added portions are also twinned in a corresponding manner, the *twinning bands cutting directly across both the cores and the new bordering material.*

The proof that these are really enlargements of hornblende fragments is, then, much the same as that hitherto advanced



One millimeter.

of the enlargements of quartz and feldspar. The more important of these are indicated by the figures in the accompanying plate.

Figures 1 and 2 are enlarged rounded grains of hornblende. The curved lines represent the broad lines of division between the old and new hornblende. This line is not an unbroken, sharply continuous one, but is built up of small gas cavities and particles of ferrite. With a low power the ferrites and cavities make an apparently unbroken line, but with a higher power their true character is plain. The sharply serrate out-

lines of the enlargements are noticeable. At points where the lines are not thus serrate the grains have grown until in contact with other grains of hornblende or feldspar. It will be noticed that the cleavage is continuous in places from the inner into the outer hornblende, while at times these cleavage lines break abruptly at the division line between the old and new material. This fact alone is an almost decisive proof that the exterior and interior parts of the individuals are of the same nature, and that the growth of the whole has been an interrupted one. The regular, worn, rounded outlines of the interior parts show that this interruption has been a great one. It will be noticed that the enlargements are narrow or wanting in the direction transverse to this cleavage. This cleavage direction, as is well known, is that in which hornblende individuals commonly have their greatest length. It is most often the case that the entire enlargements have occurred in the direction of the greatest magnitude of the particles of hornblende.

Figure 3 shows an enlarged, crystal-outlined grain of hornblende. These crystal-outlined grains are sparsely present in many of the sections from the conglomerate at Ogishke Muncie and Cacaquabic Lakes, but are commonly enlarged. The enlargements themselves never have crystal outlines. How it chances that such grains occur in a clastic rock, I leave for another to discuss. In figures 1, 2, and 3 all lines are structure or form lines.

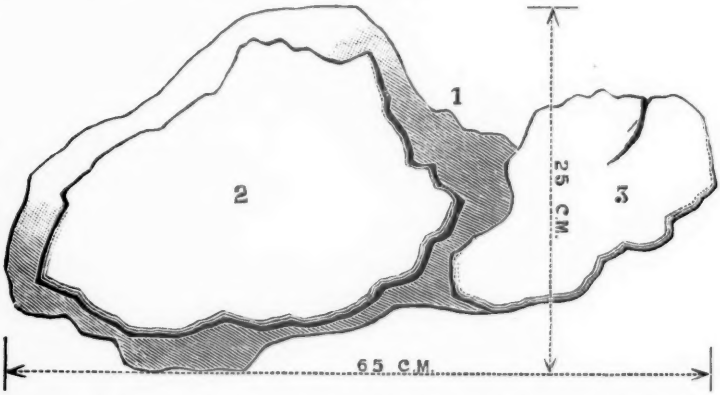
Figures 4 and 5 represent enlarged twinned crystals of hornblende. Parts of like shading in each case represent parts which become dark simultaneously in polarized light. It is plain that twinning bands cut through the lines which separate the clastic cores and newly formed hornblende. The cleavage lines, which are not shown in the figures, are parallel to the greatest length of the grains. These grains then also represent the tendency to enlarge in this direction as mentioned above. They also show finely the sharply angular outlines of the added hornblende.

ART. XXXI.—*On three Masses of Meteoric Iron from Glorieta Mountain, near Canoncito, Sante Fe County, New Mexico;* by GEORGE F. KUNZ. With four Plates.

THE meteorite, described in this paper, was discovered by Mr. Charles Sponsler, a prospector, and was supposed by him to be a mineral of peculiar value. It was found on some unclaimed land on Glorieta Mountain, about half a mile from a house in the woods, one mile northeast of Canoncito, Sante Fe County, in May (?), 1884. The mass was lying on a rock,

upon which it had fallen, in three fragments, and judging from the few marks of weathering, had not been long exposed. The exact date of the discovery I am unable to give at this time, for since it came into my possession I have not succeeded in communicating with Mr. Sponsler, he being absent out of the reach of the mails. In due time I expect to be able to settle this point and to announce it with other facts of interest.

The weight of the entire mass is 317 lbs. (143·76 kilos.). Perhaps one kilo had been chipped off before it came into my possession, so that the original weight was probably about 145 kilos. The diagram accompanying this will give a general idea of the former relative position of the three pieces; No. 1 projecting above and below No. 2, as indicated by the shading, and No. 3 fitting in at the lower right hand end. The dimen-



sions of the whole were approximately as follows: length 25 inches (65 cm.), height 10 inches (25 cm.), thickness 15 inches (37 cm.) It is curious that so large and compact a mass of iron should have been so completely broken asunder, and in this respect the fall is quite unique. The fractures are very clean considering the size of the fragments, although the edges are somewhat irregular. No. 1 is filled with elongated hollows, proving that it evidently was disturbed, and the twistings in No. 2 at the point of impact would lead to the conclusion that the falling body was partly semiplastic; but Professor R. H. Thurston, who kindly examined the iron, compares the fracture to the effect that is produced by a sudden heavy blow on cold iron and has observed the same violent wrenching in an iron target used in heavy gunning practice and now at the Stevens Institute, Hoboken, N. J.

No. 1 weighs 148½ lbs. About one-third of the whole sur-

face shows the disjuncture very plainly, as also the exact point where this began. The mass measures 15½ inches (39 cm.) in length, 12 inches (30 cm.) in width, and 8¾ inches (22 cm.) at the thickest part, and at the thinner 5 inches (12 cm.) One portion has a peculiar bubbled pasty appearance, as if the mass had been cooled in water at this point. Some of the depressions on the surface, or pittings, are 5 cm. across, and quite deep and well-marked. The upper figure on plate V represents the torn side of this mass. Plate VI shows the Widmannstätten figures produced by etching a surface of a fragment cut from No. 1; this plate is printed from an impression taken directly from the etched slab.

No. 2 weighs 115 lbs. (52·38 kilos.), and measures 16½ inches (41 cm.) in length, 10 inches (24 cm.) in width, and 6½ inches (16 cm.) in thickness. About one-third of the surface of this piece shows the remarkable rupture, the remainder being covered with the pittings. On one corner there is a portion, 10 inches by 6, which is evidently the spot where the mass struck the rock. Here the pittings are flattened and the whole mass distorted and curled over, giving it a radiated or fan-like appearance. The front, or pitted, side of No. 2 is well exhibited in Plate III, and the torn side in Plate V.

No. 3 weighs 53½ lbs. (24·263 kilos.), and measures 12 inches (30 cm.) in length, 8½ inches (21¼ cm.) in height, and 6 inches (15 cm.) in thickness in the thickest part. Over five-sixths of the entire surface is pitted, some of the depressions being 5 cm. across and nearly 2 cm. deep. The place of rupture is plain, and the iron here is coarsely fibrous, possibly because it was farther from the point of impact. There is also a fissure about 4 inches (10 cm.) deep and nearly 1 cm. wide, opposite the broken face (see plate IV). In this fissure are two broken ends of chisels which were broken in the attempt to pry off this piece, and which may have enlarged the opening. The front side of No. 3 is shown in Plate IV.

This iron is one of the Holosiderites of Daubrée, and comes under the general group of Caillite of Meunier; it is related to the irons of Augusta County, Virginia, Whitfield County, Georgia, and Washington County, Wisconsin. The iron is of characteristic octahedral structure, and the Widmannstätten figures are made up of kamacite (Balkeneisen or beam-iron), i. e. iron with little nickel, enveloped in tænite (Bandeisen), rich in nickel, and plessite (Fülleisen).* On the single cut made, one field of dark plessite measured 17 mm. by 8 mm., the kamacite from ·5 mm. to 2 mm. in breadth. The tænite was abundant and brilliant.

*Die Meteoriten Sammlung des k. k. mineralogischen Hof-Kabinetes in Wien. Am. 1. Mai, 1885, by Dr. Aristides Brezina; (Separat-Abdruck aus dem Jahrbuch der k. k. geol. Reichsanstalt, for 1885, vol. xxxv, No. 1).

The following is the result of an analysis of a compact piece of iron from No. 3, made by Mr. James B. Mackintosh, E. M. of the School of Mines, New York City.

Fe	87·93
Ni	11·15
Co	0·33
P	0·36
	99·77

Carbon, sulphur and other constituents were not determined. The specific gravity of the entire mass No. 2 was taken on a common steel-yard, and found to be 7·66+. The figures may be of interest, as showing the homogeneity of the mass, although the method was not delicate.

Troilite was observed in several places on the crust of No. 1, also traces of olivine, and perhaps schreibersite? But as yet the investigation of the mass is incomplete; all facts regarding the included minerals will appear in a succeeding note on this fall, together with some approximate data as to the velocity at the moment of impact.

I will add that this meteorite was brought to my notice by Messrs. F. Alfred Reichardt & Co., and Dr. H. G. Torrey, who have courteously given me information on the subject.

SCIENTIFIC INTELLIGENCE.

I. Physics.

1. *On "Transfer-resistance" in Electrolytic and Voltaic Cells;* by G. Gore. (Abstract.)—The existence of this phenomenon has been a matter of doubt ever since the year 1831, and the question has been examined by many investigators. In the present paper are described a series of methods by means of which its reality has been determined. Other methods are given for measuring the amounts of such "resistance," either collectively at the two electrodes of an electrolytic cell, or separately at each electrode. Modes of obviating the interference of polarization, and of securing success in the measurements, are also described.

The influence of various circumstances upon the phenomenon were investigated, viz: strength and density of current; total resistance; density of current and size of electrode; composition of the electrolyte; strength of the same; combined electrolytic cells; temperature; and chemical corrosion. The relations of the phenomenon to size of plate in voltaic cells, to the positive and negative plates respectively, and to strength of current in those cells, were also examined, and the results are given.

The following are the chief facts established by this research: That a species of electric "resistance," distinct from that of polarization and of ordinary conduction-resistance, varying greatly in amount in different cases, exists at the surfaces of mutual contact of metals and liquids in electrolytic and voltaic cells. That this "resistance" varies largely in amount with different metals in the same solution, and with the same metals in different solutions, in dilute solutions of mineral acids of different strengths, or of different temperatures; and is usually small with easily corrodible metals which form quickly soluble salts, and large with those which are not corroded; and is disguised in the case of those which by corrosion form insoluble salts.

The results of the experiments also show that the same voltaic current was "resisted" in different degrees by every different metal when employed as an anode, and when used as a cathode; also by the same metal when used as an anode and cathode respectively; and that the proportions of such "resistance" at an anode and cathode of the same metal, varied with every different metal in every different electrolyte (and strength of electrolyte), and at every different temperature; and that the resistance at the anode was usually smaller than that at the cathode; in some cases, however, where a film was formed upon the anode, an apparently reverse effect occurred; that a current from a given positive plate of a voltaic cell was differently resisted by every different metal used as a negative plate in that cell; and that by rise of temperature "transfer-resistance" was usually and considerably reduced.

They further show that this species of "resistance" was largely reduced by increasing the strength of current; and was thus conspicuously distinguished from ordinary conduction-resistance of the electrolyte. In consequence of this effect, "transfer-resistance" was greatly influenced by every circumstance which altered the ordinary resistance, and thereby the strength of current. The usual effect of diminishing the density of current alone, by enlarging both the electrodes and keeping the strength constant, was to diminish the "transfer-resistance;" and of enlarging one only, was to diminish it at that electrode and increase it at the other, the effect being greatest at the altered electrode; but the influence of density was very much smaller than that of strength of current. The current was usually less "resisted," and larger with a small positive plate and a large negative one, than with those sizes reversed. Alterations of size or kind of metal at one plate of an electrolytic or voltaic cell affected the "transfer-resistance" at the other, by altering the strength and density of the current.

"Transfer-resistance," therefore, appears to vary, not only with every physical and chemical change in the metals and liquids, but also with every alteration in the current. Such "resistance" throws light upon the relative functions of the positive and negative plates of voltaic cells, and illustrates the comparatively small influence of the negative one in producing strength of cur-

rent. Nearly all these conclusions are based upon results represented by average numbers obtained by series of experiments.—*Proc. Roy. Soc.*, No. 236.

2. *Electrical Resistance of the new alloy Platinoid.*—The new alloy Platinoid is essentially German silver with the addition of 1 or 2 per cent of metallic tungsten. It has a specific gravity of 8·78 (at 20° C.); the color is white, and when polished is hardly distinguishable in appearance from silver; it is especially remarkable for being practically untarnishable, resisting to great degree the ordinary tarnishing effect of the air. BOTTOMLEY has carried on a series of experiments having as their object the determination of its electrical resistance. He finds that it possesses the same properties of high specific resistance and small variation of resistance with change of temperature that make German silver wire so suitable for the galvanometer and resistance coils, only in a higher degree. The specific resistance of platinoid is about one and a half times that of German silver. The average percentage variation of resistance per 1° C. between 0° and 100° was found to be 0·02087 in one case and 0·022 in another; the corresponding values, as obtained by Matthiessen are at 20°, for copper 0·388, platinum-silver alloy 0·031, gold-silver alloy 0·065 and German silver 0·044.—*Proc. Roy. Soc.*., No. 237.

3. *Annual change of the Aurora Borealis.*—A recent number of *Nature* contains an interesting review of a work by Tromholt on the Aurora. After remarking the fact that Weyprecht was the first to advance the view that the auroral zone is farthest south at the equinoxes and farthest north at the solstices, the following quotation in regard to this point is given from Tromholt.

"My researches have led me to endorse Weyprecht's theory. I feel satisfied that the Aurora Borealis moves toward the autumnal equinox southward, and then northward, reaching its farthest northern limit about solstice. After this it again moves southward, being in its most southern position at the vernal equinox, when the movement is again in a northerly direction.

"From this it follows that the two maxima occurring in the temperate zone at the equinoxes must approach each other more the farther north the point of observation is situated. This is, in fact, the case. As some examples, I may mention that, while the two maxima occur in March and September in St. Petersburg, Åbo, Stockholm, Christiania, Worcester (Mass.), and New Haven, they occur in February and October in Aalesund, Newberry, Quebec, and Newfoundland; in December to January in Hammerfest, and in January at Fort Reliance. Very instructive in this respect are also the observations from the three Greenland stations: Upernivik, Jacobshavn, and Ivigtut. At Ivigtut, the southernmost of the stations, the yearly maximum must certainly be said to occur in January, but there is a second maximum toward the autumnal equinox. At Jacobshavn, eight degrees farther north, there is but one distinctly marked maximum in January, and at

Upernivik the northernmost of the stations, the maximum falls at the winter solstice more marked and dominant than anywhere else in the world."

4. *Note on a preliminary Comparison between the Dates of Cyclonic Storms in Great Britain and those of Magnetic Disturbances at the Kew Observatory;* by BALFOUR STEWART and WM. LANT CARPENTER.—We took the dates of thirty storms from Mr. Scott's paper entitled "The Equinoctial Gales; do they occur in the British Isles?" in the "Quarterly Journal of the Meteorological Society" for October, 1884, and by the kindness of Mr. Whipple, of the New Observatory, were enabled to make the comparison mentioned above.

Out of these thirty storms, in twenty-three cases there is a distinct magnetic disturbance, for the most part preceding the storm by somewhat more than a day. We do not, however, imagine that we have thus proved the fact of such a connection, but think the results we have attained sufficient to justify us in pursuing the subject.—*Proc. Roy. Soc.*, No. 236.

5. *Properties of Matter;* by P. G. TAIT. 320 pp. crown 8vo. Edinburgh, 1885 (Adam and Charles Black).—The excellent volumes by Professor Tait on the subjects of Heat and Light, already noticed in this Journal, are now followed by a third on the Properties of Matter, and, as we are informed, the series is to be completed by three others on Dynamics, Sound, and Electricity. The value of such works as these to the student of physical science can hardly be overestimated. Instead of the dull mechanical style of many of the older text-books, only enlivened by the too elaborate illustrations, these volumes of Professor Tait are fresh, bright and suggestive, and calculated to tempt a student on to do independent thinking for himself. The present work is especially welcome since it is devoted to a series of topics which have not often been discussed connectedly in a single volume.

6. *The Mathematical Theory of Electricity and Magnetism;* by H. W. WATSON and S. H. BURBURY. Vol. i, Electrostatics. 268 pp. 8vo. Oxford, 1885 (The Clarendon Press).—This volume is intended, as the authors state, as an introduction to, or commentary upon, Maxwell's work on Electricity and Magnetism. The three opening chapters are devoted to a series of introductory mathematical propositions on Green's theorem, spherical harmonics and the potential. The fundamental electrical phenomena are then briefly described, much as given by Maxwell, and afterward the usual series of topics in electrostatics discussed mathematically in succession. Students who find difficulty in reading Maxwell's larger treatise will be assisted by this volume, in which his views and methods are more or less closely followed.

II. GEOLOGY AND NATURAL HISTORY.

1. *Report of Progress of the Geological and Natural History Survey of Canada during the years* 1882–83–84, ALFRED R. C. SELWYN, Director. Montreal, 1884. (Dawson Brothers.)—Among

the valuable Reports in this volume is one on the Labrador and Hudson Bay region by Mr. ROBERT BELL. The author observed glacial striæ in many places and the following are some of the facts. About the Southern Cape of Hudson's Straits the direction (magnetic, and the same beyond) of the striæ was southeastward—S. 35° E.—though varying among the hills with the trends of the valleys; on the north side of Hudson Strait, near Cape Prince of Wales, the direction was S. 40° E. to S. 60° E.; across the south end of Nottingham Island S. 30° E. (and the bowlders were largely limestone, indicative of a limestone formation to the westward); near Marble Island on the west side of Hudson's Bay (where occurs quartzyte having often ripple-marked surfaces), the direction of the glacial striæ is S. 10° E. The remark is made in the concluding summary that on both sides of Hudson's Bay the movement of the ice was to the southward and eastward; that an extensive glacier moved eastward down Hudson Strait, which had its head in Fox's Channel and terminated in the Atlantic Ocean. Glaciers are said to exist now in this channel and to be the source of the small icebergs that float down the strait. Mr. Bell also concludes that throughout the Glacial period "the top of the coast range of Labrador stood above the ice and was not glaciated, especially the high northern part." In the southern part of the Labrador peninsula the general course of movement "appears to have been southward, varying to the eastward and westward with the courses of the valleys;" but over Newfoundland "from the center toward the sea on all sides." The rocks met with along Hudson Strait and Bay were mainly Archæan. But on Mansfield Island and Cape Southampton, to the west, a fossiliferous limestone formation was seen, which was probably Lower Silurian.

In the report of Mr. R. W. ELLS, on the Gaspé Peninsula, it is stated that the later investigations show that much the larger part of the island instead of being under Triassic rocks, is Permo-Carboniferous, as shown by the fossil plants collected at various places; and that the part of the coal formation affording the great coal beds of Cape Breton, Pictou and Spring Hill is probably wanting.

Mr. A. P. Low describes the rock of a ridge on the Gaspé peninsula, including the prominent peak Mt. Albert, averaging two and a half miles in breadth, as an olivine rock, more or less changed to a dark green serpentine. Chromic iron occurs in the serpentine, but not in sufficient quantities for profitable mining.

Mr. R. CHALMERS has a report on the Quaternary geology of Western New Brunswick. The directions of the glacial striæ (corrected for variation) in Carleton and York Counties were mostly between S. 15° E. and S. 30° E. The many lake-basins of the region are attributed to the partial filling of preglacial valleys by drift during the Glacial period, and a subsequent scooping out by local glaciers and currents. Terraces, kames and other glacial phenomena of the region are described.

The Report of Mr. HUGH FLETCHER on the Geology of Northern Cape Breton gives interesting facts with regard to the relations of the Carboniferous beds to the associated pre-Cambrian rocks. The volume is accompanied by a series of colored maps of the Province of Nova Scotia to illustrate a report by Mr. Fletcher of 1879 to 1884.

The Reports of Mr. G. M. Dawson on the region of Bow and Belly Rivers, and of Mr. Hoffmann on the coals and lignites of the northwest have already been noticed in this Journal.

2. *Aralo-Caspian and Mediterranean Basins.*—A paper on the Inland Seas and Salt Lakes of the Glacial period, by Mr. T. P. JAMIESON, is contained in the Geological Magazine for last May (III, ii, 193). It illustrates the great extension of such seas during the progress and decline of the era of ice by a review of the facts connected with the Great Salt Lake of Utah, the Dead Sea, the Aralo-Caspian Basin, the Pangong Lake and the Mediterranean Basin.

Speaking of the *Aralo-Caspian Basin* it states that the Caspian is 84 feet below the Black Sea, and that a rise of 107 feet would cause its waters to flow westward into that sea; that a rise of 220 feet would make the Caspian waters to flow northward into the Tobolsk and down the Obi into the Arctic Ocean. Its former recent connection with that ocean is sustained by the existence in its region of mollusks, crustaceans, fishes, the Beluga and seals, some of the species closely like or identical with Arctic kinds. The seal, *Phoca caspica*, is by some made a variety of the Arctic *P. vitulina*. The fishes include the sturgeon, herring, sterlet and salmon; the crustaceans, *Idotea entomon* and *Mysis relicta*, both Arctic species.

With regard to the Mediterranean, Mr. Jamieson, after referring to the opinion that a land communication must have existed between Spain and Africa during some part of the Quaternary, and that this would become a fact by a rise of a thousand feet, since a ridge crosses from Cape Spartel to Cape Trafalgar with no greater depth above it than 167 fathoms, urges that not only migrations across from Africa to Spain and the rest of Europe would thus have become possible, but also, in his view, migrations to Malta, Sicily and other islands within the Mediterranean Basin. He argues that the sea without an outlet would lose its water by evaporation, inasmuch as the average rainfall within the watershed is but 30 inches, while the evaporation is stated (Encyl. Brit., Art. Mediterranean) to exceed 60 inches, until an equilibrium was established between the loss and the supply, and that in this way the sea would be reduced to two or more lakes. He speaks of a dry climate intervening between the two Glacial eras of Europe, and favoring such a result. Thus there would have been made a dry path over to Sicily for African elephants of two species, two also of hippotamus, and other species, and for a similar migration to Malta; a dry way also, for foxes to Minorca, and hares, martens, deer, foxes, etc., to Corsica and

Sardinia. The depth of water between Africa and Sicily is now about 200 fathoms.

This theory of making dry passage-ways over the Mediterranean bottom and lakes of the salt sea requires elevation only at Gibraltar. The relations between the present salt-water life of the sea and that of Glacial and pre-Glacial time, found fossil in shore-deposits and elevated beds, ought to decide its merits.

3. *Union Group, Pacific Ocean.*—In an article on the composition of some coral limestones, etc. from the South Sea Islands, by A. Liversidge, published in the Journal of the Royal Society of New South Wales for 1880, p. 181, one of the specimens analyzed is stated to have been brought by Dr. Messer, R. N. of H. M. S. Pearl, from a raised reef on the Duke of York Island, one of the Union Group, *at a height of* 110 *feet above* the sea-level. There is an error here, for the Duke of York Island is a low atoll, and so are the others of the Union Group, the greatest elevation being 14 or 15 feet.

The analysis afforded 1·97 per cent of alumina (with traces of iron sesquioxide) and 0·789 of silica, and the amount of alumina suggests that the coral-reef rock may have come from some one of the barrier-islands of the ocean. The whole analysis is as follows: Carbonic acid 41·68, lime 52·09, magnesia 0·86, potash 0·98, soda 0·85, alumina and traces of iron sesquioxide 1·97, manganese *trace*, silica 0·79, organic matter 0·50, chlorine *trace*, hygroscopic moisture ·02=99·75. J. D. D.

4. *Spiraxis major and Sp. Randalli of Newberry; large Screw-like fossils from the Chemung group of Northern Pennsylvania and Southern New York.*—These singular fossils are described and figured by Dr. J. S. Newberry in the Annals of the N. Y. Academy of Sciences, vol. iii, no. 7, p. 217. They are three to seven inches long, tapering slightly in either direction from the middle, half an inch to over an inch in greatest diameter, and are marked with a broad and deep spiral groove. But they are without any trace of organic structure, and Dr. Newberry describes the specimens and mentions possible biological relations, without venturing any decided opinion.

5. *Geological Map of the United States;* by W. J. McGee, of the U. S. Geological Survey.—This map, although a small one (18×28 inches), will be found of great value to the geological student. It has been carefully prepared and is colored in the best style of the art.

6. *Macfarlane's Geological Railway Guide.*—Dr. James Macfarlane is preparing a new edition of his valuable Geological Railway Guide, corrected to correspond with the recent discoveries, which will soon be published by Appleton & Co. The part for Canada has already been issued.

7. *Impact Friction and Faulting;* by George F. Becker.—The following misprints occur in the portion of this article which appeared in the August number:

Page 120, line 3 from top, $dw = wdx$ should read $dw = -wdx$.
Page 121, line 2 from top, for W read w.
Page 122, line 8 from top, for mv_2 read mv^2.
Page 123, line 7 from bottom, for *average* read *energy*.
Page 127, the figure should be as follows:

8. *Plantes à Fourmis.*—Under this title M. Levier has reproduced in the French language, in the *Archives Italiennes de Biologie*, the substance of Beccari's *Pianti Ospitatrici, ossia Piante Formicarie della Malesia e della Papusia*, (that of the introductory portion), from the second volume of Beccari's *Malesia*. The first part of this, with its copious figures, is devoted to the consideration of plants which hospitably shelter and in part feed colonies of ants in a peculiar and quasi-pathological growth. The first-known plant of this kind is *Acacia cornigera* of Central America. Hernandez, about the middle of the seventeenth century, described the huge stipular thorns of this tree and the way in which ants of a particular species eat into them from the apex, feed upon the pulpy interior substance, and make there their dwelling, feeding also upon the sweet secretion of the leaf-glands. As Belt (The Naturalist in Nicaragua) describes, the stipular thorns or horns, thus inhabited, grow still larger and in a different shape from those of the species which are unoccupied; and the ants are said to pay for their food and lodging by effectually keeping off herbivorous animals and other species of insects which otherwise would attack the tree. The ant-inhabited plants next made known were two woody *Rubiaceæ*, of Sumatra, a *Hydnophytum* and a *Myrmecodia*, which, in 1750, Rumphius described and illustrated, the first as *Nidus formicarum niger*, being inhabited by a kind of black ant, the second as *Nidus formicarum ruber*, because it harbored a red ant. Both shrubs are epiphytic, and both make a large tuber-like growth at the base where the attachment to the foster-tree takes place;—an abnormal and pathological growth, in the sense that the development is stimulated and aggrandized by the irritation of the colonizing ants which find food and lodging in the cavernous interior, yet natural because the tuberous enlargement begins in the germination of the plant before the ants attack it. It is the caulicle, or hypocotyledonous stem, which thus swells out when the embryo germinates, and this appears to form the whole of the large tuber. All this is beautifully illustrated by Beccari, in plates 8, 13, and 21 of the second volume of Malesia; where it is moreover shown that the swelling caulicle becomes hollow by its own growth. So it appears that these insect-colonized Rubiaceæ prepare in advance for their peculiar guests, although the amplitude of the lodging is doubtless increased by their subsequent action. Indeed Beccari states that the seedling plants which fail to be inhabited soon cease to grow. This, however, contradicts the

opinion of Treub, that the ants have nothing to do with the formation of this organ. Be this as it may, it is most supposable that this extraordinary formation was acquired gradually; that the normally fleshy caulicle of the ancestral plant, made a nidus by an insect, developed under the disturbing stimulus somewhat as a gall develops, until at length the tendency became hereditary and the singular adaptation of plant to insect was established. In formulating such an hypothesis, which falls in so naturally with prevalent conceptions, Signor Beccari, in his elaborate exposition, goes back very far for his starting point, even to the properties of protoplasm, the development of protista into protophytes and protozoa, the development of the former into higher-organized forms, and so on; coming down at length to the hypothesis that the various adaptations of flowers to insects, (irregularities in form, the development of nectaries, the growth of these into hollow spurs or sacs, etc.), have resulted from the irritant or disturbing action of visiting insects. The whole dissertation is interesting and ingenious.

Beccari, in Malesia, indicates 16 species of *Myrmecodia* and 29 of *Hydnophytum*. This association with ants is established in many of them. He also describes and figures less remarkable cases of ant-lodgings in stems thereby more or less distorted by enlargements, in a *Myristica*, in an *Endospermum* and another of the Euphorbiaceæ, a *Clerodendron*, and in three Palms of the genus *Korthalsia*. A. G.

9. *Lloyd's Drugs and Medicines of North America.* — The sixth part, issued in June, 1885, extending from p. 177 to 208, brings to a close the elaborate medical and botanical history of *Hydrastis*, with 43 bibliographical references to the botany alone. It devotes a good figure and a page of letter-press to *Trollius laxus*, and for the rest is occupied with *Coptis*, which is left unfinished. There are excellent figures and dissections of *C. trifolia* and also of *C. occidentalis* and *C. asplenifolia* of the Pacific side of the continent, and an elaborate account of the minute anatomy of *C. trifolia* by Louisa Reed Stowell, with copious and admirably drawn illustrations. The main active principle of the plant is said to be berberine. So the single order *Ranunculaceæ* is likely to fill a volume. A. G.

10. *Transactions and Proceedings of the New Zealand Institute*, for 1884, vol. xvii, 1885.—The Botanical papers, p. 214–306, show great activity among the resident botanists of New Zealand, consisting of articles by T. Kirk, W. Colenso, D. Petrie, J. Adams, H. C. Field, and W. S. Hamilton. Many new species are characterized; and there are still ample harvests to be gathered in these large and diversified islands. A. G.

11. *Revision of the North American Species of the Genus Scleria;* by N. L. Britton.—This forms pages 228–237 of the third volume of the *Annals of the New York Academy of Sciences*. As the New York Academy is a continuation and amplification of the former Lyceum of Natural History, which in

former days brought out distinguished work in *Cyperaceæ*, this continuation of its Annals upon the same line is opportune and welcome. No new species are proposed in this critical revision, but several new varieties are characterized. A. G.

12. *Beitrag zur Kenntniss der Sarraceniaceen.*—An inaugural Dissertation for the philosophical doctorate at the University of Erlangen, May, 1885, by PAUL ZIPPERER of Munich, octavo, with one double plate of anatomical detail; worked out upon *Sarracenia purpurea*, *flava*, and *variolaris*, *Darlingtonia* and *Heliamphora*. A. G.

13. CHARLES WRIGHT, our associate of many years, a veteran botanist and extensive explorer, the most kindly of men, died suddenly, of heart disease, at his home in Wethersfield, Conn., August 11th, at the age of 74. A. G.

III. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Transactions of the Connecticut Academy of Arts and Sciences*, vol. vi, Part 2. New Haven, Conn.—This part closes the volume with the 516th page. It contains the following memoirs: J. H. EMERTON, on New England Spiders of the family Epeiridæ, with 8 plates of crowded figures; and on the Lycosidæ, with 4 plates; R. H. CHITTENDEN and H. E. SMITH on the diastatic action of Saliva, as modified by various conditions, studied quantitatively; S. W. WILLISTON, on N. A. Conopidæ (conclusion); A. E. VERRILL, Third Catalogue of Mollusca recently added to the fauna of New England coast and the adjacent Atlantic, consisting mostly of deep-sea species, with notes on others previously recorded, with 4 plates; K. J. BUSH, Mollusca of Cape Hatteras, with one plate.

Professor Verrill's paper reports that the Cephalopoda here described were obtained at depths between 600 and 2574 fathoms; the Gastropoda between 43 and 2574 fathoms; and one species *Lamellaria pellucida* has this great range; the *Scaphopoda* from 70 to 1594 fathoms; the *Lamellibranchiata* at various depths down to 2221 fathoms, of which, species of Pecten occur down to 1525 fathoms, of Arca at 2021 fathoms, and of Limopsis at 2221 fathoms. Two Brachiopoda are included, one *Atretia gnomon* 1525–1594 fathoms, and *Discina Atlantica* 1198–2021 fathoms.

2. *A Catalogue of Scientific and Technical Periodicals* (1665 to 1882) *together with Chronological Tables and a Library Check-List*, by H. CARRINGTON BOLTON. 774 pp. 8vo. Washington, 1885. Smithsonian Miscellaneous Collections, No. 514.—This catalogue with its chronological tables and check list is the result of a number of years of work, and its completeness is the more remarkable that the best libraries of the world do not exist this side of the Atlantic. The volume is of great value for its carefully prepared alphabetized list of scientific and technical periodicals of all nations; but this value is very much enhanced by the addition of the Chronological Tables, which give a student

the means of finding the date of any volume cited in a reference and thus ascertaining the time of the investigation or discovery referred to. Such references ought always to have the date affixed, but, unfortunately, the great majority fail of this. The work has been handsomely and generously printed under the auspices of the Smithsonian Institution, and makes a compact and convenient volume. The science of the world owes much to Professor Bolton, and also the Smithsonian Institution for the volume.

3. *Contributions to North American Ethnology*, volume v, 4to. Washington, 1882. (U. S. Geographical and Geological Survey, J. W. POWELL in Charge.)—This important volume contains three extended memoirs, illustrated by many excellent plates. The subjects are: Observations on cup-shaped and other lapidarian sculpture in the Old World and in America, by Charles Rau, 112 pp.; On prehistoric trephining and cranial amulets, by Robert Fletcher, 30 pp.; A study of the manuscript Troano, by Cyrus Thomas, with an introduction by D. G. Brinton, pp. xxxvii and 237.

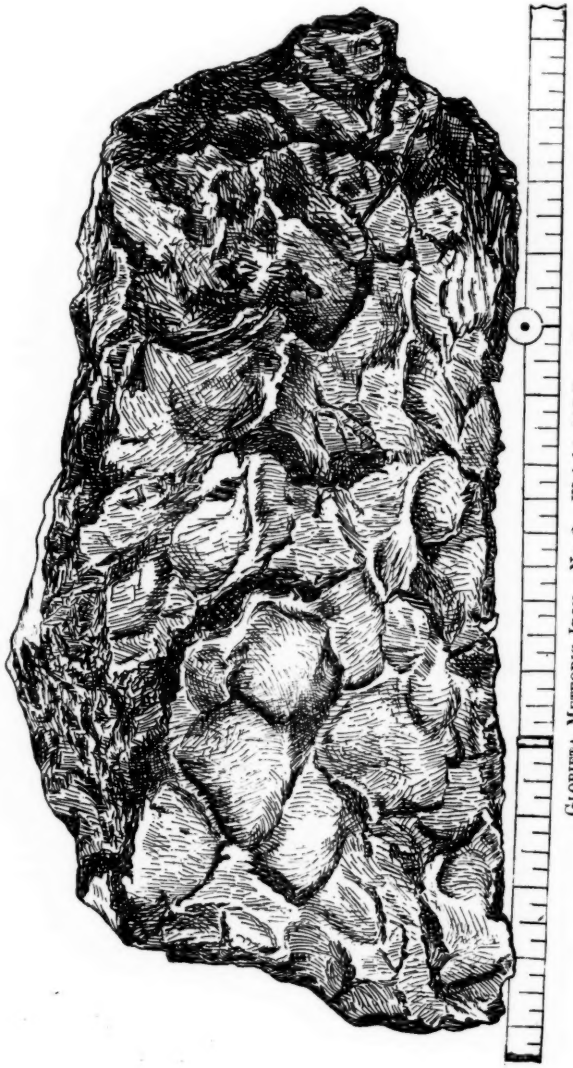
4. *The Microscope in Botany: A Guide for the Microscopic Investigation of Vegetable Substances;* from the German of Dr. JULIUS W. BEHRENS; translated and edited by Rev. A. B. Hervey, assisted by R. H. Ward. 466 pp. 8vo, Boston, 1885 (S. E. Cassino & Co.).

OBITUARY.

M. HENRI FRESCA, member of the French Academy since 1872, and distinguished for his physical researches and in mechanical engineering, died on the 21st of last June.

M. HENRI MILNE EDWARDS, the eminent zoologist of France, author of works on general zoology and the Invertebrata, and particularly the departments of crustacea and corals, died in July, in his 86th year, having been born in Belgium, at Bruges, on the twenty-third of October of the year 1800.

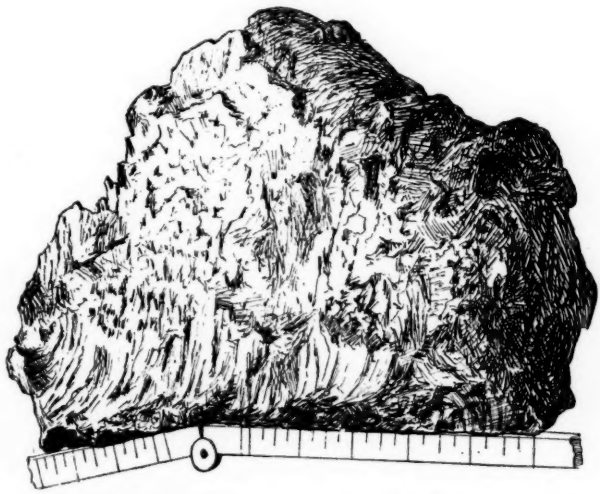
PROF. W. C. KERR, State Geologist of North Carolina for eighteen years, and more recently connected with the United States Geological Survey, died at Asheville, N. C., on the ninth of August, of consumption. Prof. Kerr was an excellent observer in geology, and in his few publications brought out results of great interest. He was the first in the country to call attention to, and rightly explain, the unequal steepness in the opposite banks of streams, where flowing through yielding deposits (Rep. Geol. N. Carolina, vol. i); and the first to appreciate adequately and describe the action of frost in producing the deep movement and bedded arrangement of loose material on slopes (this Journal, III, xxi, 1881), the depth in North Carolina being such as to indicate, in his view, the unusual conditions of a Glacial era. Owing to deficient appropriations, only one volume of the State Survey Reports has been published. He was occupied with investigations under the United States Geological Survey when his failing health brought his labors to an untimely close.



GLORIETA METEORIC IRON.—No. 2. Weight, 115 lbs. Front side.

GLORIETA METEORIC IRON.—No. 3. Weight, 53½ lbs. Front side.

GLORIETA METEORIC IRON.—No. 1. Weight, 148½ lbs. Torn side.



GLORIETA METEORIC IRON.—No. 2. Weight, 115 lbs. Torn side.

CRYSTALLINE STRUCTURE OF THE GLORIETA METEORIC IRON.
Printed from the Iron.